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ABSTRACT

The study is described as a carefully controlled field test of the effects on recipient families of eight different negative income tax or benefit formulas. The most striking finding was that observed changes in labor supply in response to the experimental payments were generally quite small. Chapter One, The Experiment: Background and Choices, reviews the policy debate in the mid-1960's, the negative income tax, and describes important considerations in the study. Chapter Two, Labor Supply Response, explains the methodology by which response variables were statistically related to treatment variables and control variables; the labor supply response--mean treatment-control differentials; and labor supply response--results of more complex models. Results are tabulated and summarized. Chapter Three presents qualifications, other findings, and further research suggestions. Data from the sample, weekly earnings, hours worked, employment rates, and percentage changes are presented in 19 tables. A list of technical papers arising from the Graduated Work Incentive Experiment and table of contents for an administrative procedures volume; abstracts of research into behavioral responses to the experiment other than the labor supply response; and an outline of the available data from the graduated work incentive experiment in New Jersey and Pennsylvania are appended. (Author/NH)

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# U.S. Department of HEALTH, EDUCATION, and WELFARE

*Summary Report:*

# NEW JERSEY GRADUATED WORK INCENTIVE EXPERIMENT

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A SOCIAL EXPERIMENT IN
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Summary Report:

# THE NEW JERSEY GRADUATED WORK INCENTIVE EXPERIMENT

Conducted by

Institute for Research on Poverty
University of Wisconsin
Madison, Wisconsin

and

Mathematica, Inc.,
Princeton, New Jersey

for The Office of Economic Opportunity

December 1973

The Graduated Work Incentive Experiment was initiated in 1967 by the Office of Economic Opportunity, through a contract with the Institute for Research on Poverty, University of Wisconsin-Madison. Field operations were conducted by Mathematica, Inc., Princeton, New Jersey. Research Director for the study is Professor Harold Watts, University of Wisconsin; Director for Operations is David Kershaw, Mathematica. The Technical Papers on which this Report is based were edited by Professor Watts and Professor Albert Rees, Princeton University. A companion volume describing the surveys, operations, and administration of the experiment was edited by Kershaw and Jerilyn Fair, Mathematica. In August, 1973, the study was transferred from OEO to the Office of Income Security Policy Research in the Office of the Assistant Secretary for Planning and Evaluation, DHEW. Inquiries pertaining to this Summary Report should be directed to this Department; inquiries pertaining to the experiment itself and the technical analysis should be directed to the Institute for Research on Poverty.

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# FOREWORD

For nearly ten years there has been debate and controversy over the effects of extending cash assistance to the "working poor" -- intact families with children headed by able-bodied, non-aged males who are currently ineligible for most public assistance programs. Central to this debate has been the question of the labor supply response of such families. Would the receipt of assistance payments cause them to work less or, in some cases, quit work altogether? Clearly, any substantial reductions in labor supply would not only increase the cost of assistance to the working poor, but would also tend to undermine the anti-poverty objectives of assistance by weakening labor market ties and reducing earned income.

It was primarily to address this question that the Office of Economic Opportunity initiated the Graduated Work Incentive Experiment in 1967. This study was a carefully controlled, scientific field test of the effects on recipient families of eight different "negative income tax" or benefit formulas. A negative income tax plan can be characterized by a "basic benefit," the amount paid to a family that has no other source of income, and an "implicit tax rate," the rate at which benefits are reduced as family income rises. The present cash welfare system and many non-cash assistance programs--e.g., food stamps and public housing--have the general benefit structure of a negative income tax; but they also include administrative restrictions and eligibility requirements which

are not necessarily present in a general negative income tax plan. The experimental plans included a broad range of levels of both basic benefits (ranging from 50 percent to 125 percent of the poverty line) and implicit tax rates (ranging from 30 percent to 70 percent).

Over 1,350 randomly selected low-income families in five New Jersey and Pennsylvania cities were enrolled in the experimental plans and a control group. The control group received no transfer payments; they were included to allow comparisons which would isolate the effects of the various payment plans. Each family remained in the experiment for three years. Intensive interviews were conducted every three months to measure a variety of family attributes, including labor supply and other behavioral responses. The first comprehensive analysis of this wealth of data has now been completed by the Institute for Research on Poverty of the University of Wisconsin and Mathematica, Inc., who conducted the study. This report summarizes the major findings of that analysis for the 693 husband-wife families who were present throughout the experiment.

The analyses reported here, like the experiment itself, are complex and multi-faceted. A wide variety of response measures were analyzed for a number of different groups of participants. For example, in the area of labor supply alone, four basic response variables were analyzed for husbands, wives, and the family as a whole, within each of three major ethnic groups. Moreover, the analytical models employed highly sophisti-

cated statistical techniques. Inevitably, the specific quantitative estimates of labor supply vary somewhat from one response measure to another, from one group of participants to another, and from one analytical model to another. The analysis of these data is an on-going process; further work is already underway at the Poverty Institute and it is our hope that other researchers will take full advantage of this unique data base. It is also our hope that the completed analyses, available in the form of a large set of detailed Technical Papers, will be carefully and critically scrutinized by other scholars, and we are taking steps to facilitate such a review.

Despite the complexity of the analyses and the diversity of the results, the broad outlines of the central labor supply results, and their importance for public policy, are now apparent. It seems unlikely that further research on this data base will significantly alter the general characteristics of these results although some caveats are discussed below.

The most striking feature of the findings is that the observed changes in labor supply in response to the experimental payments were generally quite small. For most groups of participants, the various measures of labor supply showed reductions relative to the control group of less than 10 percent; many of the differentials were much smaller, and often were not statistically different from zero. Indeed, for black families statistically significant reductions in labor supply

were virtually never found, and in a number of cases a statistically significant increase in work effort was observed. Only for wives were large percentage reductions in labor supply observed with any consistency and, again, these responses were largely confined to non-blacks. Even these responses were quite small in absolute terms; they were large only relative to the initially small amounts of labor supplied by wives. Because the labor supply of wives was small even in the absence of assistance, and because the vast majority of husbands in the experiment were employed, these reductions in wives' labor supply had only small effects on overall family labor supply and earnings. It is worth noting that over the course of the experiment, correcting for inflation, the average payment to continuous families actually declined.

It is also worth noting the form taken by those labor supply reductions which were observed, especially for husbands. It seems clear that these reductions were not the consequence of a small number of participants withdrawing from the labor force entirely to live on assistance payments. Approximately 95 percent of all husbands, in both the treatment and control groups, were in the labor force during any survey week throughout the experiment. There was no significant reduction in either labor force participation or employment rates for either white or black husbands, although whites did reduce slightly the number of hours worked per week. Only for Spanish-speaking husbands was there a statistically significant reduction in labor force participation, and it was small. The

overall reduction in labor supply among the Spanish-speaking was largely accounted for by somewhat higher unemployment rates among those in the labor force.

A large number of behavioral responses to the experimental negative tax plans outside the labor supply area have also been analyzed. These are not discussed in detail in this report, although abstracts of the Technical Papers dealing with these topics are presented in an appendix. In general, few significant responses were found in these other areas. Cash assistance at the levels involved in this study do not appear to have a systematic effect on the recipients' health, self-esteem, social integration, or perceived quality of life, among many other variables. Nor does it appear to adversely affect family composition, marital stability, or fertility rates. Perhaps these findings are not surprising, in view of the relatively modest amounts of the experimental benefits. Monthly payments averaged about $100 across all plans. Even in a plan with a basic benefit equal to the poverty line, average monthly benefits were only $125. This is because earnings of families in the experiment averaged about $450 per month. What we can say with certainty is that these benefits represented a net increase in family income, allowing these families greater command over material goods and services, and enhancing their economic well-being. The anti-poverty effectiveness of the payments was not seriously vitiated by offsetting reductions in earnings due to reduced work effort.

There are a number of caveats and qualifications which must be observed in assessing the results of this study, and a good deal of further research can be profitably pursued with these data. The temporary nature of the experiment may have had some effect on the responses observed that is different than one would expect under a permanent national program. Moreover, as with any study of a panel of families over time, there are problems of missing data and drop-outs. In addition, the existence of relatively generous AFDC-UP and General Assistance programs in the experimental sites greatly complicates the interpretation of the results. These and other problems of analysis and interpretation have been investigated in some detail by researchers at the Institute for Research on Poverty, and are the subject of continuing research. Their resolution will undoubtedly affect many of the specific findings of the study; however, the Poverty Institute's investigations suggest that the overall results of the study are valid.

Thus, they would appear to have important implications for public policy. They clearly indicate that a negative tax type plan with a basic benefit as high as the official poverty line will not trigger large-scale reductions in work effort among male heads of families. Indeed, there is no evidence here that even a small proportion of male heads would drop out of the labor force completely in response to such a plan; small labor supply reductions are likely to be evenly spread over large

numbers of workers. Without a mandatory work requirement the male heads of families maintained high levels of labor force participation under all of the experimental plans.

It does seem likely, on the basis of these results, that a national income-conditioned cash assistance plan would result in a rather substantial (percentage) reduction in the labor supply of the 15-20 percent of low-income wives who are employed. Whether this is viewed as an undesirable outcome depends on one's social values. On the one hand, it is true that a second paycheck can be the route out of poverty for many low-income families. On the other hand, there may be important costs to low-income families, their children, and society as a whole, when these women work outside the home due to economic necessity. In any event, the income security provided by such income supplementation enhances the freedom of individual women to choose their own balance between work inside or outside the home.

Even the small increases in unemployment rates among some male heads observed in the experiment are not an unambiguously undesirable outcome. There is some evidence that, especially for younger workers, these reflect longer periods of search between jobs, resulting in better jobs and higher wage rates. Thus, these supply responses should not be viewed as negatively as we would unemployment caused by insufficient demand.

Since benefits depend on family earnings and income, the cost of any given plan will be sensitive to family labor supply responses, and

particularly, earnings. Total earnings of these families tend to be dominated by those of the head, since wives tend to work few hours at low wages. Thus, the experimental results indicate that only small changes in family earnings, with only minor cost implications, should be expected in response to a negative income tax type plan. Offsetting these would be the potential for substantially reducing income poverty, increasing the command of the poor over material goods and services, and enhancing their freedom to choose among economic options.

William A. Morrill
Assistant Secretary for
Planning and Evaluation
DHEW

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## I. The Experiment: Background & Choices

### The Policy Debate in the Mid-1960s

In 1964, the President declared a War on Poverty. The goal--to increase the well-being of low-income people-dictated that attention be paid to a principal form of income for the poor, transfer payments, and their obverse, tax payments. It was generally known that the poor paid at least their proportional share of taxes and that government could help the poor by relieving them of some portion of their positive taxes. However, ways to lessen the tax burden for low-income persons were seen as limited. State and local sales and property taxes and payroll taxes for social insurance were widely regarded as regressive, but are not easily modified to reduce their impact on the poor.

In the mid-1960s, one commission and advisory group after another produced recommendations for various income maintenance plans. In October, 1965, the Office of Economic Opportunity included in its submittal to the Bureau of the Budget a negative income tax as the centerpiece of a comprehensive national anti-poverty plan. In 1967, the President of the United States announced his intention to appoint a Commission on Income Maintenance Programs.

Throughout debates on these policy alternatives ran one recurring question: Would extension of cash assistance to the low-income "working poor" substantially reduce incentives to work? Both common sense and economic theory suggested that people might work less if they were given money. But how much less?

The cost of any national transfer program would be very dependent on how much less people would work. Existing estimates of the cost of such a program varied anywhere from $3 or $4 billion to $75 billion as specification of the income maintenance plans changed, and as estimates of the amount people would alter their work behavior varied from almost not at all to quitting work altogether.

Beginning with the Poor Law debates in England there has been a substantial literature on the question of work disincentives. Little empirical analysis existed, however, to test the validity of varying conclusions. Advances in data gathering and statistical techniques over the past fifteen years have made such analysis more feasible, but even so the data available have suffered from two major defects: (1) they have not had as their major focus the low-income members of the population, and a traditional argument has been that the poor act in some way differently from the rest of society; and (2) in the real world from which such data are drawn, few members of the working poor receive income maintenance benefits. Thus, on the basis of existing data, only indirect inferences could be made as to the effects of such plans if they were to be adopted as national policy.

Then, in 1966, it was suggested that an experiment be carried out to yield direct observations on this issue of whether, and how much, people would reduce their work effort in response to cash transfers. This suggestion was received favorably both inside and outside OEO for three main reasons: (1) the government was searching for a cost-effective policy to make low-income people "better off;" (2) academic economists were coming to the realization that reliable estimates of labor-supply effects were

not forthcoming from static survey-type data; and (3) the social science research community was developing increasing interest in the possibility of extending experimentation into the area of social policy.

OEO officials were persuaded by these considerations to do a social science experiment into the effects of income maintenance on incentives to work (i.e., on labor supply). The questions then became: What kind of income maintenance program, and what kind of experiment?

The Negative Income Tax

In 1967, as now, policy interest centered on transfer programs which would restrict benefits to the low-income population, and would provide the largest benefits to those families with the least income. That is, it was agreed that transfers should be income-conditioned. The simplest form of income conditioned transfer program is one in which a "basic benefit" is provided for recipients with no other income, and benefits are gradually reduced as the recipient's income rises. The rate at which benefits are reduced as income rises may be termed the "implicit tax rate" in the program. For example, in a program which reduced benefits by $.60 for each $1.00 of additional income, the implicit tax rate is 60%. At some point under such programs, the recipient's income reaches a level at which benefits are reduced to zero by the implicit tax rate; this point is known as the "breakeven level" of income. Individuals below the breakeven level receive program benefits; those above it do not.

An example of an income-conditioned transfer program of this general form, with a basic benefit of $3000 and an implicit tax rate of 50%, is shown in the table below:

| Family Earnings | Implicit Tax Rate | Transfer Payment (Basic benefit minus earnings times implicit tax rate) | Total Family Income |
|---|---|---|---|
| 0 | 50% | $3,000 | $3,000 |
| 10 | " | 2,995 | 3,005 |
| 100 | " | 2,950 | 3,050 |
| 1,000 | " | 2,500 | 3,500 |
| 2,000 | " | 2,000 | 4,000 |
| 3,000 | " | 1,500 | 4,500 |
| 6,000 | " | 0 | 6,000 |

As can be seen from this example, increases in earnings always make the recipient family better off under such a program, because total family income increases with earnings. As earnings, and total family income, rise to the breakeven level ($6000 in this example) transfer payments are smoothly reduced to zero.

This general form of income-conditioned transfer program has come to be known as a "negative income tax." Most of the existing public assistance programs, including Aid to Families with Dependent Children (AFDC). Supplementary Security Income (SSI), food stamps, public housing, and others, have benefit structures of the negative income tax form. The Family Assistance Plan (FAP), proposed by the President in 1969, was also a negative income tax; as revised in 1970, FAP would have provided a

basic benefit of $2400 (for a family of four) with an implicit tax rate on earnings of 67%.

The negative income tax is thus a very generally applicable form of income-conditioned transfer program. By varying the basic benefit and implicit tax rate, any of a wide range of benefit structures can be achieved. Because of its generality, it was the form selected for study in 1967 when this experiment was being planned.

Although as noted above, a family is always better off by working because total family income rises with earnings, the negative income tax contains some potential, at least in theory, for reducing labor supply as compared with a situation in which the family receives no assistance. This is true for two reasons. First, the basic benefit raises the family's spendable income. This may be expected to lead to consumption of more goods and services, including leisure -- one "purchases" leisure by reducing work hours, thereby foregoing income. In effect, the recipient may decide to use part of the transfer to replace income he previously obtained by working. Second, the implicit tax rate has the effect of reducing the net value of additional earnings to the recipient family. As seen in the example, an additional $1.00 of earnings leads to an increase in total family income of only $.50 if the implicit tax rate is 50%. This could also have a tendency to reduce labor supply.

Some reduction in benefits as earnings rise is, of course, unavoidable in any income maintenance program which is intended to restrict benefits to families below a specified income level. The question in 1967, as now,

was how rapidly benefits could be reduced (i.e., how high the implicit tax rate could be set) without creating a serious work disincentive--or if, indeed, any acceptable benefit reduction formula could be found. Experimental analysis of alternative income-conditioned transfer plans promised to provide reliable empirical estimates of both of these work disincentive effects.

A Controlled Social Science Experiment

Any of three major kinds of field tests could have been chosen. The first can best be described as a demonstration. This is simply a project to show that a program can work. It contains no control group and often has no systematic evaluation procedures. The second can be called a pilot. It is a program that has been approved for adoption which is implemented on a small scale to begin with, in order that lessons can be learned as to how to implement it best, and how to avoid administrative errors. The third--which is the one chosen by OEO in 1967--is the controlled experiment. Its purpose is to measure the behavioral response of a group or groups to a certain, carefully defined, "treatment." Such an experiment involves a systematic statistical design, and includes a "control" group that does not receive the experimental treatment but is like the treatment group in every other possible way, to allow comparisons which enable analysts to isolate affects which are due solely to the treatment.

In setting up the controlled experiment in negative income taxation, three major issues had to be resolved: (1) From which population should

the sample be drawn? (2) Where should the experiment be located? (3) What specific experimental treatments should be used?

(1) From which population should the sample be drawn? In 1967, the population group about which least was known, and for which people expected the greatest disincentive, was the so-called "working poor." This group is composed primarily of intact families headed by non-aged, able-bodied males with dependents. The working poor have historically received almost no transfers from the American welfare system.* From both the experimental and policy points of view, they are the families most likely to have observable changes in their labor supply in response to cash transfers. It was decided, therefore, that although a major part of the recipient population in any national transfer program would be the traditionally eligible public assistance population--female-headed families, the disabled, and other categorical groups--the most important group to obtain information about first was that group about whose potential withdrawal of labor least was known, and whose labor supply is the largest. (It should be noted that, for the existing welfare population in 1967, a negative income tax with implicit tax rates of less than

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* The only existing federally-funded cash transfer program for which intact working poor families are eligible is the Unemployed Parent segment of Aid to Families with Dependent Children (AFDC-UP). To be initially eligible for AFDC-UP benefits, a male family head must be totally unemployed and have exhausted any unemployment compensation to which he is entitled. The program therefore covers only a small proportion of the working poor. Moreover, AFDC-UP programs have only been adopted by about 30 States.

100% would have increased work incentives, because at that time welfare recipients were subject to a 100% implicit tax rate on net earnings.)

The alternative of trying to put together a sample that would represent a microcosm of the whole potentially eligible population was rejected on the grounds that it would run the risk of lumping together such diverse groups that, within the limits of a reasonable sample size and budget limitation, the information gained would not be sufficient to yield reliable results about any of the subgroups. It was assumed in the planning discussions in 1967 that the problem of representativeness would be solved by a series of income maintenance experiments, each of which would focus on different subgroups of the potentially eligible population. (Some of these groups other than intact families are now being studied in other experiments initiated subsequently.)

In addition to the restriction on type of family, the sample was also restricted to the low-income groups in the population. To be eligible for participation in either the treatment or control groups, a family had to have an income of not more than 150 percent of the poverty line. In 1967, this cutoff level was approximately $5000 for a family of four. The income cutoff was deliberately set above the poverty line itself, first because some plausible negative income tax plans have breakeven levels above the poverty line, and second because theory and common sense both indicate that response to an income-conditioned transfer program may not be restricted simply to those eligible at the start of the program, but might also affect those above the breakeven level initially who reduce

their earnings in order to qualify, or whose income subsequently is reduced for reasons beyond their control.

(2) Where should the experiment be located? Here, again, a decision was made to limit the type of family included--this time, geographically. Although there were some reasons to favor a dispersed national sample, this alternative was rejected primarily on operational grounds. Large-scale social experimentation was a largely untried methodology and there was real doubt as to whether an experiment involving a dispersed national sample could, in fact, be administered satisfactorily. It was thus decided to restrict the experiment to one geographical area.

Why New Jersey and Pennsylvania? The urban industrial center cities in general contained a significant enough proportion of the working poor who would be newly eligible under a national transfer program to make them a necessary category of interest. The Northeast was chosen as a fairly restricted geographical area in which a large number of the residents were central city inhabitants. The State of New Jersey was chosen in part because, at the time the site selection was being made, New Jersey was virtually the only major Northeastern industrial State which had no AFDC-UP program--the principal welfare program for which intact families are eligible. It was thus anticipated that the choice of New Jersey would minimize the danger of the results being confounded by competing welfare alternatives. As it turned out, in January 1969, the month in which the second site was being activated, New Jersey intro-

duced a generous AFDC-UP program, and this advantage was lost. (The impact of the existence of the AFDC-UP alternative on the experimental results is discussed in Section III below.)

It was not originally anticipated that experimental sites would be selected in other states. However, when it became clear that the eligible population in the sites chosen in New Jersey did not contain enough non-Spanish-speaking whites to enable the sample to maintain an ethnic balance, it became necessary to look further for a population of eligible whites. This consideration led to the selection of Scranton, Pennsylvania as an additional experimental site.

(3) What experimental treatments should be used? As was explained above, a negative income tax plan is composed of a basic benefit and an implicit tax rate. A decision, therefore, had to be made as to what magnitudes of these two plan parameters to use in the experiment. Policy interest in 1967 centered around implicit tax rates in the neighborhood of 50 percent, and basic benefits of around 75 percent of the poverty line. The experimental plans were therefore designed to bracket this area of policy interest. It was also thought important to employ a sufficiently broad range of plans to allow measurement of response to variation in the plan parameters. Eight negative income tax plans were finally chosen (combinations of three tax rates and four basic benefit levels), as follows:

| Plan | Basic Benefit (Percent of poverty line) | Basic Benefit for a Family of Four (Dollars in 1968) | Implicit Tax Rate (Percent) |
|---|---|---|---|
| I | 50 | $1650 | 30 |
| II | 50 | 1650 | 50 |
| III | 75 | 2475 | 30 |
| IV | 75 | 2475 | 50 |
| V | 75 | 2475 | 70 |
| VI | 100 | 3300 | 50 |
| VII | 100 | 3300 | 70 |
| VIII | 125 | 4125 | 50 |
| Control Group | 0 | 0 | 0 |

Basic benefits under each of these plans were adjusted for family size, in proportion to the family size adjustments employed in the official poverty line. In addition, over the course of the experiment, basic benefits were adjusted annually for changes in the Consumer Price Index. Eligible families were identified within a randomly selected sample of residents of poverty areas in the cities chosen as sites. This pool of eligible families was then randomly assigned to the various plans and the control group, in accordance with a statistical design which determined the number of families in each income stratum to be assigned to each group.

There were 1,216 families originally enrolled in the experiment--725 in the treatment groups and 491 in the control group. They were enrolled sequentially in four sites, as follows:

August, 1968 --Trenton, New Jersey
January, 1969 --Paterson and Passaic, New Jersey
June, 1969 --Jersey City, New Jersey
September, 1969 --Scranton, Pennsylvania

In October, 1969, 141 additional families in Trenton, Paterson, and Passaic were added to the control group.

The operation of the experiment lasted for three years in each site. During the three-year period, the families in the treatment groups and the control group were administered an hour-long interview every three months. In addition every four weeks each family in the treatment groups filled out a Income Report Form on the basis of which the payments for the families were calculated. Payments were recalculated every four weeks, and the family received the indicated amount in bi-weekly checks. Payments averaged about $23 per week across all plans, with weekly averages of $5 in the least generous plan and about $50 in the most generous. In comparison, weekly family earnings averaged about $115. (The allocation of families among plans and the control group, average payment levels, and average values of various measures of labor supply are presented in Appendix A.)

The operational phase of the experiment was completed late in 1972. Over the past year intensive efforts have been devoted to data preparation

and editing, and estimation of analytical models. A number of separate studies have now been completed, and are available at reproduction cost from the Institute for Research on Poverty, at the University of Wisconsin. (A listing of these Technical Papers is provided as Appendix B.) The following section of this Report summarizes the major findings of these studies with respect to labor supply response. Section III discusses several qualifications of the data and findings, and briefly describes the nature of the results in areas other than labor supply, as well as ongoing and future research with these data. (Abstracts of those studies which have been completed are presented as Appendix C; a description of the data base itself is provided as Appendix D.)

## II. Labor Supply Response

The New Jersey experiment was a complex research undertaking which produced massive amounts of data. It should be expected that the results are complex, somewhat ambiguous, and difficult to summarize. This is the case. Indeed, there is no one result; there are many. For example, the principal goal of the experiment was to measure labor-supply response. But labor supply is not a one-dimensional concept and in the discussion that follows findings pertaining to four different measures of labor supply will be discussed. Nor is the population studied homogeneous either in demographic characteristics or in responses across demographic groups. Significant differences were continually found among the three ethnic groups: white, black, and Spanish-speaking Americans.

While there is great diversity in the results of this complicated, sophisticated and on-going statistical analysis there is also sufficient uniformity to allow certain significant policy inferences to be drawn. The previous sentence may seem somewhat paradoxical. Recall, however, that the purpose of the experiment was to determine if income-conditioned transfers would have a substantial effect upon labor supply. The results for different groups and different measures of labor supply can vary considerably, while at the same time few of the results for particular groups be large and/or statistically significant. It is the latter sort of overall finding that allows a relevant policy inference to be drawn.

Statistical Methodology

The purpose of the analysis is to explain variations in several dimensions of labor supply: labor force participation, employment, hours worked, and family earnings. In order to do so these response variables are statistically related* to two other types of variables: treatment variables and control variables.

The "treatment" can be viewed as eligibility for experimental payments, that is, being in the treatment group, as opposed to being a "control" family which receives no payments. Or the treatment may be considered to be the payment level, which depends on the plan to which the family is assigned, as well as the family's size and income. Finally, the level of the implicit tax rate (30%, 50%, 70%) and the

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* The data from the experiment were analyzed by regression analysis, a statistical technique for estimating relationships among several variables. The estimates show, on average, the values of these relationships in the population studies and provide estimates of the variability of these relationships within that population. A particularly useful property of regression analysis in the context of anything as complex as this experiment is that it permits the analyst to control or hold constant a wide range of influences while estimating a particular relation. For example, in estimating the effect of NIT payments on the differential in hours worked between experimental and control families, it is useful to include as control variables the values of such attributes as pre-enrollment hours worked, location, ethnicity, age, family-size, education, occupation, and industry. The experimental effects on hours worked estimated from such a regression equation may be interpreted as applicable to control and experimental groups with identical composition in terms of these variables.

basic benefit (50%, 75%, 100%, and 125% of the poverty line) may be used directly as variables measuring the experimental treatment. In each case, the size and statistical significance* of the relationship between the response variable and the treatment variable allow us to determine how labor supply is affected by the negative income tax plans in the experiment. Below, an effect of the treatment on a measure of labor supply will be referred to as a treatment effect or experimental effect.

All of the effects discussed will be in the form of differences between the value of a response variable (hours worked per week, for example) for "treatment families" and the value of the same variable over the same period for "controls." A treatment family is one which received (or was eligible to receive) payments. A "control" is a member of the control group, which did not receive any payments. The existence of the control group allows the analysis to abstract from events in the particular site and from the point in time that the experiment was conducted. This is because both treatment and control families

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* In this report when relationships or effects are termed "significant," formal statistical significance is implied. This concept may be defined as follows: The data in the experiment were collected from a sample of the population. Within such a sample, no matter how careful the sampling procedures, there is some unknown degree of random error in the observed relationships and there is always some possibility that estimated relationships occurred by chance. Statistical theory allows one to determine the probability that a given result could have occurred by chance. Thus, when it is said that a particular relationships between labor supply and an experimental treatment is "significant," it is meant that the probability that the estimated relationship could have occurred by chance is less than a specified probability level. The probability levels used in this report are no greater than five percent and are frequently less.

were subject to identical labor market and other external conditions. Thus, for example, experimentals and controls lived in geographical areas with the same employment rate.

In order to take account of any systematic differences between treatment and control families, a large set of control variables was typically used in the statistical analysis. In part, these were necessary to control for differences between the treatment and control groups resulting from stratified random assignment. In part, however, their inclusion was in response to the fact that even in a simple design it is important to control for systematic differences that may survive the randomization process. The purpose of the control variables is to ensure that any relationships found between the response variables and the treatment are just that and not the result of systematic differences between treatment and control families in terms of factors such as, for example, age, health, education, or, in the case of the labor supply of wives, the number and ages of children. Thus any remaining differences between treatment and control families should represent only those factors with respect to which the two groups differ: namely, the experimental treatments.

The results will be presented for three categories of participants--husband, wife, and the family as a whole. The analysis reported here is based on data from "continuous husband-wife families"--two-parent families who completed at least eight of the quarterly interviews. There are 693 such families in the sample, out of an initial sample of 1216 families, and a total of 1003 of these same families remaining at the end of the experiment. This subset of families was selected for discussion here

because they are a relatively homogeneous group representing the modal family type among the working poor, for whom the analysis is not complicated by the problems of changes in family composition and widely varying degrees of missing data which characterize the rest of the sample. A comparison of the full sample and the subset of continuous husband-wife families, in terms of characteristics at pre-enrollment and allocation by plan, site, ethnicity, and income level, is presented in Appendix A, Tables 1 and 2.

The analyses described here are based on data from the "central two years" of the experiment; that is, the third through the tenth quarterly interviews. This data set was selected in order to avoid the possibility that the experimental effects would be confounded with any transitory effects arising from the beginning of the experiment (e.g., "learning effects") or the anticipation of its termination. The full three-year data set is also being analyzed by the Institute for Research on Poverty, with particular attention to identification of any transitory effects.

Two sets of results are described below. They differ in the definition of the treatment variables and in the form of the analytical models estimated. Quantitative estimates are presented for the first set of results, which are based upon a relatively simple model. These estimates provide the most straight-forward summary measures of the impact on labor supply of the experimental transfer plans. The second set of results, based upon more sophisticated, complex models, provides estimates of the experimental effects which are, in general, more detailed

and precise. Full explication of these models is beyond the scope of this Summary Report. The discussion of these results will therefore focus primarily on those areas where the more refined estimates differ, in magnitude or significance, from the results of the simpler models. Complete specifications of these models are contained in the Technical Papers.

Labor Supply Response--Mean Treatment-Control Differentials

The simplest measure of experimental effects is the difference in mean labor supply between treatment families and control families. The treatment-control differentials reported here were estimated by regression analysis, including as control variables age, education, number of adults, number and ages of children, sites, and pre-experiment family earnings and labor supply.

Tables 1-3 show, for husbands, wives and the entire family, the control family mean, the treatment family mean and the absolute and percentage differentials between these two means for four different measures of labor supply. Negative differentials indicate smaller labor supply on the part of treatment families as compared with control families. Within each Table, results are reported separately for each ethnic group.

The most striking features of the results for husbands, shown in Table 1, are that all of the differentials are quite small in both absolute and relative terms--none exceed 10 percent of the control mean and most are less than five percent--and all are statistically

insignificant (I.e., one cannot rule out the possibility that these differentials occurred purely by chance).* There are no findings here to indicate a significant reduction in labor supply resulting from the experimental payments. Moreover, many of the differentials, including all of those for blacks, are positive, indicating greater labor supply among husbands in the treatment group than in the control group. Finally, it is worth noting that the means for both groups indicate that the vast majority (approximately 95 percent) of the husbands were labor force participants who when employed worked close to full-time (37 to 40 hours per week).

The results for wives, are presented in Table 2, showed predominantly negative labor supply differentials. These were small in absolute magnitude, but, because of the low levels of market labor supply of wives, these differentials represent relatively large percentage differentials--at least for white and Spanish-speaking wives.** Even so, only two of the differentials shown in the Table--those for labor force participation

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* See fn, page 16.

** The means presented in the tables are averages over <u>all</u> individuals within a given group, including non-workers. Corresponding means for workers only can be readily calculated from the numbers presented. For example, while all white wives worked an average of 4.5 hours per week, the 17.1 percent of the control group who were employed worked an average of 26.3 (4.5/.171) hours per week.

and employment rates of white wives--are statistically significant. This lack of significance reflects the small absolute size of the differentials and the small sample sizes of working wives in each of the three ethnic groups; for example, in any given survey week there were only about 15 working wives among the Spanish-Speaking families in the entire sample.

Mean labor supply differentials for the family as a whole, shown in Table 3, were preponderantly negative, but again were relatively small.* In no case do the differentials exceed 14 percent of the control mean, and most are less than 10 percent. All of the differentials for white families except for the earnings measure are statistically significant, while none of those for black or Spanish-speaking families are significant.

In summary, these results present a picture of generally small absolute labor supply differentials between the treatment and control groups as a whole. Only among wives, whose mean labor supply is quite small to begin with, are the differentials large in relative terms. While a number of the differentials are positive, contrary to theoretical expectations, great confidence cannot be placed in the specific numerical values because of their statistical insignificance. That is, one cannot rule out the possibility that these differentials occurred by chance

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* Family means and differentials include the labor supply of all workers in the family, not just husband and wife.

Table 1

Husband Totals: Regression
Estimates of Differentials in Labor Force Participation, Employment, Hours, and Earnings for Quarters 3 to 10[a]

| | Labor Force Participation Rate | Employment Rate | Hours worked per week | Earnings per week |
|---|---|---|---|---|
| **White** | | | | |
| Control group mean | 94.3 | 87.8 | 34.8 | 100.4 |
| Absolute differential | -.3 | -2.3 | -1.9 | .1 |
| Treatment group mean | 94.0 | 85.5 | 32.9 | 100.5 |
| Percent differential | -.3 | -2.6 | -5.6 | .1 |
| **Black** | | | | |
| Control group mean | 95.6 | 85.6 | 31.9 | 93.4 |
| Absolute differential | 0 | .8 | .7 | 8.7 |
| Treatment group mean | 95.6 | 86.4 | 32.6 | 102.1 |
| Percent differential | 0 | .9 | 2.3 | 9.3 |
| **Spanish-speaking** | | | | |
| Control group mean | 95.2 | 89.5 | 34.3 | 92.2 |
| Absolute differential | 1.6 | -2.4 | -.2 | 5.9 |
| Treatment group mean | 96.8 | 87.1 | 34.1 | 98.1 |
| Percent differential | 1.6 | -2.7 | -.7 | 6.4 |

(See next page for footnotes)

Table 1

[a] The data for these tables consist of 693 husband-wife families who reported for at least 8 of the 13 quarters when interviews were obtained. The reported differentials in each measure of labor supply are the experimental treatment group mean minus the control group mean, as measured in a regression equation in which the following variables were controlled: age of husband, education of husband, number of adults, number of children, sites, pre-experiment labor supply variables of the husband. These means and the associated control-treatment differentials may therefore be interpreted as applicable to control and treatment groups with identical composition in terms of these variables. Percent differentials are computed using the mean of the control as base.

[b] Official government labor force concepts, used in the experiment, define someone as in the labor force if he is employed or unemployed. Someone is unemployed if he is actively seeking employment, waiting recall from layoff or waiting to report to a new wage or salary job.

Table 2

Wife Totals: Regression
Estimates of Differentials in Labor Force Participation, Employment, Hours, and Earnings for Quarters 3 to 10[a]

| | Labor Force Participation Rate | Employment Rate | Hours worked per week | Earnings per week |
|---|---|---|---|---|
| White | | | | |
| Control group mean | 20.1 | 17.1 | 4.5 | 9.3 |
| Absolute differential | -6.7* | -5.9* | -1.4 | -3.1 |
| Treatment group mean | 13.4 | 11.2 | 3.1 | 6.2 |
| Percent differential | -33.2 | -34.7 | -30.6 | -33.2 |
| Black | | | | |
| Control group mean | 21.1 | 16.8 | 5.0 | 10.6 |
| Absolute differential | -.8 | -.3 | -.1 | .8 |
| Treatment group mean | 20.3 | 16.5 | 4.9 | 11.4 |
| Percent differential | -3.6 | -1.5 | -2.2 | 7.8 |
| Spanish-speaking | | | | |
| Control group mean | 11.8 | 10.7 | 3.4 | 7.4 |
| Absolute differential | -3.8 | -5.2 | -1.9 | -4.1 |
| Treatment group mean | 8.0 | 5.5 | 1.5 | 3.3 |
| Percent differential | -31.8 | -48.3 | -55.4 | -54.7 |

(See footnotes next page)

Table 2

[a]The data for these tables consist of 693 husband-wife families who reported for at least 8 of the 13 quarters when interviews were obtained. The reported differentials in each measure of labor supply are the experimental treatment group mean minus the control group mean, as measured in a regression equation in which the following variables were controlled: age of wife, education of wife, number of adults, number and ages of children, sites, pre-experiment family earnings (other than wife's) and pre-experiment labor supply variables of the wife. These means and the associated control-treatment differentials may therefore be interpreted as applicable to control and treatment groups with identical composition in terms of these variables. Percent differentials are computed using the mean of the control as base.

[b]Official government labor force concepts, used in the experiment, define someone as in the labor force if he is employed or unemployed. Someone is unemployed if he is actively seeking employment, waiting recall from layoff or waiting to report to a new wage or salary job.

*Significant at the .95 level (two-tailed test).

Table 3

Family Totals: Regression
Estimates of Differentials in Labor Force Participation, Employment, Hours, and Earnings for Quarters 3 to 10[a]

| | Number in labor force per family[b] | Number employed per family | Hours worked per week | Earnings per week | Percent of adults in the labor force, per family | Percent of adults employed per family |
|---|---|---|---|---|---|---|
| White | | | | | | |
| Control group mean | 1.49 | 1.30 | 46.2 | 124.0 | 57.6 | 51.1 |
| Absolute differential | -.15** | -.18** | -6.2** | -10.1 | -5.3** | -6.1** |
| Treatment group mean | 1.34 | 1.12 | 40.0 | 113.9 | 52.3 | 45.0 |
| Percent differential | -9.8 | -13.9 | -13.4 | -8.1 | -9.1 | -12.0 |
| Black | | | | | | |
| Control group mean | 1.38 | 1.17 | 41.7 | 114.0 | 54.3 | 46.9 |
| Absolute differential | -.07 | -.07 | -2.2 | 4.1 | -1.6 | -1.6 |
| Treatment group mean | 1.31 | 1.10 | 39.5 | 118.1 | 52.7 | 45.3 |
| Percent differential | -5.4 | -6.1 | -5.2 | 3.6 | -2.9 | -3.3 |
| Spanish-speaking | | | | | | |
| Control group mean | 1.15 | 1.04 | 39.0 | 102.4 | 48.9 | 44.7 |
| Absolute differential | .08 | -.02 | -.4 | 5.0 | 2.4 | -1.0 |
| Treatment group mean | 1.23 | 1.02 | 38.6 | 107.4 | 51.3 | 43.7 |
| Percent differential | 6.7 | -1.5 | -.9 | 4.9 | 5.0 | -2.2 |

(See footnotes next page)

Table 3

[a]The data for these tables consist of 693 husband-wife families who reported for at least 8 of the 13 quarters when interviews were obtained. The reported differentials in each measure of labor supply are the experimental treatment group minus the control group mean, as measured in a regression equation in which the following variables were controlled: age of husband, education of husband, education of wife, number of adults, number and ages of children, sites, and pre-experiment labor supply variables for the husband and wife. These means and the associated control-experimental differentials may therefore be interpreted as applicable to control and experimental groups with identical composition in terms of these variables. Percent differentials are computed using the mean of the control as base.

[b]Official government labor force concepts, used in the experiment, define someone as in the labor force if he is employed or unemployed. Someone is unemployed if he is actively seeking employment, waiting recall from layoff or waiting to report to a new wage or salary job.

**Significant at the .99 level (two-tailed test).

and that there was no systematic treatment effect on labor supply. All of the statistically significant differentials are negative.

While the treatment-control differentials discussed above provide a useful summary measure of the overall impact of the experimental treatments on labor supply, this analysis is not as detailed or precise as one might wish. First these differentials are based on a simplistic definition of the experimental treatment--namely, the mere presence of experimental payments, without any distinction among the eight different negative income tax plans employed in the experiment. The experimental effects implied by these differentials, then, reflect an average response to a rather heterogeneous mixture of treatments. Second, these differentials represent an average of responses across widely differing families. A more refined analysis would allow for the likelihood that the response would vary systematically with family characteristics; thus, for example, one might expect that families whose labor supply was low to begin with would respond differently than families with high initial labor supply.

The models analyzed in the Technical Papers take these refinements into account. They allow for different responses to plans which vary both in basic benefit levels and implicit tax rates. They also allow the response to vary with such family characteristics as income level and length of time in the experiment. These results are discussed in this section, with emphasis on those findings which differ from the

responses implied by the simple treatment-control mean differentials in labor supply reported above.

Husbands. Differences in work behavior between treatment and control husbands were small. Save for the fact that no sizable work effort reduction appeared, however, no clear behavior pattern was revealed.

Looking first at labor force participation* we find no significant treatment effect for whites and blacks. The treatment did, however, result in a small but statistically significant decrease in labor force participation on the part of Spanish-speaking husbands. Turning to hours worked per week, significant treatment effects were again found for Spanish-speaking husbands. If one evaluates the estimated response function** for an average Spanish-speaking husband on a plan with a basic benefit equal to the poverty line and a 50% implicit tax rate, the treatment effect on weekly hours worked is a reduction of 3.2 hours (mean hours

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* Official government labor force concepts, used in the experiment define someone as in the labor force if he is employed or unemployed. Someone is unemployed if he is actively seeking employment, waiting recall from layoff or waiting to report to a new wage or salary job.

** The "response functions" on which the results presented in this section are based are regression equations relating the labor supply response variables to a set of control variables and the basic benefit levels and implicit tax rates of the experimental plans. These regressions were estimated using data from all continuous husband-wife families, in all plans and the control group. By inserting specific values of the control and treatment variables in these equations, one can predict the labor supply response of a particular type of family on a particular plan. In this section references to responses under a specific plan are based on this type of calculation. In general, these predictions will be more precise than those based only on data from families in a particular plan.

worked by Spanish-speaking control husbands were 34.3). A similar calculation for white husbands yields a statistically significant reduction of 2.4 hours per week. For black husbands there was once again no significant treatment effect.

Much of the reduction in hours among Spanish-speaking husbands can be accounted for by declines in their employment rate (that is, the fraction of all Spanish-speaking husbands in the experimental population who were employed). This implies that Spanish-speaking husbands were unemployed more when in the treatment group,* a result which is given independent confirmation when data on unemployment are analyzed directly. For white husbands, whose hours were reduced as noted above, the employment effect was small (and positive) so that all of the experimental effect would appear to be in hours worked per week for those at work. As yet we do not know if this result arises from less overtime work, a reduction in multiple job holding, or some other source.

Viewing the results by experimental plan, it was found that the reduced labor supply for Spanish-speaking husbands varied, as we would expect, with the implicit tax rate--higher implicit tax rates produced substantially stronger disincentives. For whites the reverse was true--the largest disincentives were estimated for plans with the lowest implicit

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* To see this, note first that treatment-control differences in labor force participation were small. Labor force participation includes employment plus unemployment. Thus, if husbands in the treatment group have a lower employment rate but about the same labor force participation as controls, the difference must be accounted for by a higher unemployment rate among those in the treatment group.

tax rates. In neither case was there a strong or consistent ordering by basic benefit level; indeed, the most generous plan (125-50) showed the smallest treatment effects. Overall, then, the experiment produced no consistently significant effects by implicit tax rate or basic benefit. These results do not, of course, allow prediction of the labor supply effects of implicit tax rates or basic benefits outside the range employed in the experiment--that is, implicit tax rates below 30% or above 70%, or basic benefits less than 50%, or greater than 125%, of the poverty line.

By far the most surprising result of the analysis for husbands is the complete failure to find any significant effect for blacks, despite the fact that black husband-wife families received slightly larger average payments than the other two ethnic groups. Indeed, the estimated supply response for blacks is not only insiginficant, but preponderantly positive. This kind of finding for blacks is not limited to husbands; it recurs in the analysis of other components of the household. We have no plausible explanation for this outcome. The data indicate that earnings of the black control group increased more slowly over the course of the experiment than those of the other control and treatment groups. Thus when treatment-control comparisons are made for blacks the differential in favor of the treatment group is noticeably large. We do not know why this is so although there is always some possibility that the result arises from sampling variability.

To summarize the results for husbands: significant reductions in hours were found for white and Spanish-speaking husbands, with those

for whites being quite small absolutely. Only for the Spanish-speaking husbands was there a significant treatment effect on labor force participation. Finally, no significant labor supply response of any kind was found for blacks.

These results, while diverse and sometimes inconsistent with theoretical expectations, do shed substantial light on one of the policy questions the experiment set out to answer. The effect of income-conditioned cash transfer programs on the labor supply of male family heads has been a prime concern in discussions of welfare reform since the mid-1960s. To the extent that the results of the experiment can be generalized to the national low-income population, they indicate that a national program of income-conditioned transfers, at the benefit levels considered here, would have only relatively small effects on the labor supply of male family heads. (The generalizability of the results is discussed in Section III below and in Part C of the Technical Papers.)

Wives. The results for wives are discussed in terms of labor force participation and hours worked. It is important to note that the labor supply of wives in the experiment as reflected by both of these measures, particularly labor force participation, are well below their average values for the population as a whole. For example, the pre-enrollment labor force participation rates of 16.0 percent and 13.4 percent for treatment and control wives, respectively, are less than one-half their values for all married women in the population. This results from the way in which the sample was selected. Only families with income less than

one and one-half times the poverty line were admitted to the sample. Therefore, families with multiple earners had a low probability of selection. In addition, because the poverty line is adjusted upward as family size increases, the higher-income families in the experiment were likely to have larger families and younger children. Both of these factors lead to an underrepresentation of working wives. Because pre-enrollment labor supply was quite small the absolute differentials seem large indeed in percentage terms. We shall return to this point below.

The analysis showed significant negative effects on labor-force participation rates for all wives in the treatment group, taken as a whole. When the results are disaggregated by ethnic group, they are seen to arise almost entirely from the behavior of white wives--the estimated effects of the treatment on the participation rates of blacks are close to zero and sometimes positive and the estimates for Spanish-speaking wives are unstable and never significant. When the response variable is hours worked, the results are generally similar, but somewhat weaker. The estimated effect for black wives is positive and significantly different from that of white wives. Once again we have no explanation for the strong differences in results by ethnic group.

In distinguishing among experimental plans, responses were generally consistent with expectations. For all wives the estimated negative response is consistently larger the higher the more generous the plan, and the differences in response by plan are usually significant. A

similar comparison by implicit tax rates found larger effects the higher the implicit tax rate, but these differences were usually small and never significant.

The estimated effects on labor supply of wives are subject to two rather different interpretations. The average estimated reduction in labor-force participation for all wives referred to above is 3 percentage points; for white wives it is 8 percentage points. These do not represent large absolute changes taken alone. But, because the mean participation rate for all control wives is only 17 percent, the estimated percentage reduction in labor supply for all wives in the treatment group (compared to controls) is 20 percent, and, for white wives, it is a sizeable 50 percent.

The results thus indicate that income-conditioned cash transfers may cause a substantial percentage reduction in the proportion of working wives, at least among white wives with large families. How such a result is evaluated in terms of social priorities will depend on one's views about the value of having mothers remain in the home. The average number of children in the sample families was approximately four. Wives in families of this size provide valuable services in the home. Viewed as a reallocation of wives' total work effort between the home and the outside labor market, reductions of several hours per week in the market work of wives look less substantial.

It should also be noted that these estimated effects may be larger than those to be expected in an otherwise similar, but permanent, income maintenance program. For the control families, no more than 19 percent of wives

were in the labor force in any one quarter, but 41 percent were in the labor force in at least one of the 13 quarters (counting pre-enrollment). In other words, this is a group that enters and leaves the labor force frequently. The experimental treatment creates a strong incentive to concentrate periods out of the labor force during the life of the experiment. A permanent program might therefore be expected to have a somewhat smaller impact.

The Family. The analysis summarized here covers the family as a whole, including husbands, wives, and all other members of the household 16 years of age and over. In addition to hours worked by the family, family earnings is used as a response variable. Variations in earnings are particularly important since transfer payments to a family depend on family earnings. If a labor supply decline should cause an earnings reduction, program costs would rise. Another desirable property of earnings as a labor supply measure is that it provides a natural way to value or weight the hours worked by different family members; the weight is the wage rate of each member.

Unfortunately, however, there is a possible bias in the use of the earnings variable not present in the other measures. Treatment families filled out an income report form every four weeks, while control families did not. The treatment families may therefore have learned more quickly than control families that what was to be furnished was gross rather than net earnings (that is, earnings before taxes and other deductions, not take-home pay). If this were the case, since gross earnings exceed

net earnings in the treatment group would appear greater, relative to control earnings, than they actually are. This differential learning process could have caused a spurious differential in earnings in favor of the treatment group, especially during the early part of the experiment. Therefore, the results for hours worked and labor-force participation may be more reliable than for earnings.

Hours worked and earnings both showed a significant reduction for white families, ranging from 8 to 16 percent for hours and 8 to 12 percent for earnings. For blacks, the earnings effects are significantly positive, rising by 9 to 13 percent. Effects on hours worked by black families are small and show no consistent pattern; in one analysis a decline of 3 percent was found, while in another an increase of 1 percent appeared.

For Spanish-speaking families estimates of significant hours reductions in the neighborhood of 2 percent to 6 percent were found, while earnings were estimated to fall from 2 percent to 28 percent. These estimates are based on evaluation of the estimated response functions for families in plans with a 50 percent implicit tax rate.

In parts of the analysis the statistically predicted variance of family income was included as a control variable. This variable represents the fluctuation in income over time--for example, from $200 per month in February to $600 per month in July for a construction laborer. Such a variable was included for two reasons. First, families with variable income may have weaker attachments to the labor force, and therefore the

experimental payments may have a stronger effect on their behavior. Second, variation in income gives the family experience with the effect of the implicit tax rate on the level of payments. This variance of income measure had a highly significant effect on the labor supply of whites. The more variable was income, the more labor supply declined. Other ethnic groups did not evidence such behavior.

The results for white families are thus consistent with those from the separate analyses of husbands and wives in that significant negative effects on labor supply are found. For blacks, the results again show predominantly positive responses, though not consistently so for hours worked. For Spanish-speaking families, the labor supply effects are negative, though generally smaller and less significant than for whites.

Summary

In general, the estimated effects of the experimental negative income tax plans on labor supply are in accord with theoretical expectations. The major surprise is the absence of any negative effect on the labor supply of black households. For white and Spanish-speaking families, and for the experimental group as a whole, the effects are negative, usually statistically significant, but not very large. They consist primarily of a reduction in hours worked of white husbands, an increase in the unemployment rate of Spanish-speaking husbands, and a large relative reduction in the labor -force participation rate of white wives.

If the results found by ethnic group were applied to the national low-income urban population, given its ethnic composition, then the relative importance of the response of whites would rise and the importance of the response of Spanish-speaking families would fall. Of course, any such extrapolation to national estimates is risky; it is not at all clear, for example, that results for Puerto Ricans in New Jersey say anything at all about the behavior of Spanish-speaking Americans of Mexican descent in the Southwest.

We place less weight on the results for blacks for a different reason. These appear to arise in large part from the unusual behavior of the black control group, whose labor supply and especially earnings fell relative to other control groups for reasons we do not understand. That the experimental treatment effects for blacks are often statistically significant is not an assurance that they are not biased.

The patterns of labor supply response found in the experiment are not as clear as might have been expected. Yet in many ways they are clearer and more sensible than the results of much of the nonexperimental literature.* Certainly they call into serious question the very large effects estimated in some of the nonexperimental studies. On the basis of the experimental results, it does not appear that income-conditioned cash transfers for intact families at the levels of basic benefits and implicit tax rates employed in the experiment would have very large effects on labor supply.

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* The nonexperimental literature on work incentives uses cross-sectional data such as the Census or the Survey of Economic Opportunity to estimate (via regression analysis) the relation between hours of work or labor force participation and wage rates, non-labor income and a host of control variables. The estimated relations between labor supply and wage rates are used as an estimate of the implicit tax rate effect of a negative income tax and the non-labor income effect is used as an estimate of the basic benefit effect.

A very serious problem with this procedure is that the non-labor income variable generally includes some income which is in fact directly related to employment status (e.g., unemployment compensation, welfare, or pensions) and in any case is an imperfect proxy for the basic benefit in a negative income tax. In general, the data from an experiment designed to measure transfer program effects are almost certainly far superior, in that response to an actual transfer program can be measured, as opposed to simulating responses.

The results of studies using non-experimental data are not inconsistent with the results of the experiment, although the latter are toward the low end of the range of work disincentive estimates. The non-experimental estimates of labor supply reduction in response to a negative income tax range from about 3 percent to 18 percent, with only a few studies providing higher estimates. For wives the range is from 20 percent to 50 percent.

For critical survey of the non-experimental literature see Irwin Garfinkel, "Income Transfer Programs and Work Effort: A Review, "Studies in Public Welfare, Paper No. 13, Subcommittee on Fiscal Policy Joint Economic Committee, U. S. Congress (forthcoming).

## III. Qualifications, Other Findings, and Further Research

### Qualifications

While the results of the experiment probably provide the most reliable estimates to date of the effects of income-conditioned cash transfers on the labor supply behavior of low-income families, several qualifications must be borne in mind in assessing these findings.

First, it must be emphasized that the sample was not designed to be a nationally representative cross-section of the low-income population. Rather, it is a random sample of families headed by non-aged, able-bodied males, drawn from four urban areas of the Northeast, whose family incomes at enrollment were less than 150% of the poverty line. As compared with the national population of non-aged, male-headed low-income families, for example, the experimental sample contains larger proportions of non-whites, large families, and young family heads. Throughout the analysis, therefore, it was necessary to control statistically for those characteristics of the sample which may be non-representative and which may influence labor supply behavior. While these statistical procedures complicate somewhat the presentation of the results, they do greatly enhance the generalizability of the findings.

A detailed comparison of the attributes of the sample and of the experimental sites with national data is contained in Part C.IV of the Technical Papers. While a number of differences are apparent, the sample and the experimental sites as a group do appear to be reasonably representative of the urban U.S., and particularly the non-South urban U.S., in terms of most characteristics of labor force behavior and labor market conditions.

Nevertheless, caution should be exercised in generalizing specific quantitative findings from the experiment to the national population.

Secondly, generalizations from the experiment to the effects of a permanent national program must be qualified by recognition of the temporary nature of the experiment. There are theoretical reasons to expect that in a program which participants know to be temporary, the observed labor supply responses to basic benefit levels will understate, and the responses to implicit tax rates will overstate, the responses that could be expected under a permanent program*. The magnitude of these biases can be expected to depend -- in a very complex fashion -- on the duration of the experiment, the time period for which participants plan in making economic decisions, and the discount rate at which they value future income. While there is some indication that such biases are present to some degree, the existing data do not allow precise quantification of the bias. Much more precise analysis of this problem will be possible in the ongoing income maintenance experiments in Seattle and Denver, which include families enrolled for five years as well as for three years.

It should be noted, however, that since the biases in response to basic benefits and implicit tax rates operate in opposing directions, their importance is greater for assessing these responses separately than for assessing the overall response to any particular income maintenance plan. Taken together, these two biases will tend to cancel each other. Our best current estimates indicate, for example, that while the coefficients measuring responses of white families to these two policy parameters may each

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* This issue is discussed in detail in Part C. III of the Technical Papers.

be biased by as much as 20 percent, the net combined response is probably understated by less than five percent. For labor supply reductions as modest as those found in the experiment, a five percent bias is trivially small in absolute terms. Moreover, as noted above, the observed response to a temporary experiment probably overstates the labor force withdrawal to be expected among married women in a permanent program. This is probably true of other secondary earners in the family.

A third qualification which should be borne in mind is that the experiment did not simulate a program which would replace existing welfare programs. Instead, the experiment coexisted with a relatively generous welfare program (AFDC-UP) for unemployed male heads and their families. Participants were allowed to move freely between welfare and experimental payments -- although they were not allowed to receive both simultaneously. The proportion of families in the experiment receiving welfare ranged from about 9 percent to 14 percent for families in the treatment group and from 15 percent to 27 percent for controls, over the course of the experiment.

Families who chose welfare in preference to the experimental plans were not a random subset of participants -- their capacities and preferences for work almost certainly differ from those of other families in the sample. Moreover, the different levels of support provided by the various experimental treatments created different incentives to opt for welfare. This raises the possibility that the composition of the sample receiving experimental payments, relative to that of the control group, was differentially affected, introducing systematic bias into the results.

The quantitative significance of this potential bias is analyzed in

Part C.II of the Technical Papers. By applying alternative assumptions about the behavior which would have been observed in the absence of welfare, it is possible to estimate upper and lower bounds of the true Treatment-control differential in labor supply response.* These estimates indicate that the existence of welfare in the experimental sites did not have a major effect on the estimated labor supply differentials--at most, the bias is a few percentage points.

Fourth, as in any longitudinal study, there is the problem of missing data. Gaps in data arise for a variety of reasons, including non-response to individual questionnaire items, entire interviews which were missed due to inability to locate families, and families who dropped out of the experiment completely. Of the 1,216 families present at pre-enrollment, 333 missed one or more of the thirteen quarterly interviews conducted over the course of the experiment. Of these 213, or about 18% of the total sample, dropped out of the experiment completely. This is very close to the rate of attrition anticipated at the outset of the experiment,** and somewhat better than the attrition experienced in other panel studies.

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* There is no straightforward way to estimate the true differential directly. One cannot, for example, simply drop the welfare families from the sample or treat welfare as a separate experimental treatment, because welfare families are not a random subset of the sample. Either of these approaches would, therefore, also result in biased estimates.

** The experimental design was based on anticipated losses of 10 percent among families receiving large payments and 20 percent among controls and treatment families receiving minimum payments.

If data losses due to missing interviews and attrition are randomly distributed across plans and family types, they are unlikely to bias the estimated experimental response. If systematic relationships exist between missing observations and experimental treatment or family characteristics, however, the response estimates may be biased. The extent to which this is the case has been investigated in detail in Part C.I of the Technical Papers. This analysis indicates that there are indeed some systematic differences between families who remained in the experiment and those who dropped out. It is however, an exceedingly difficult and complex matter to infer from these differences whether, or to what extent, the observed responses are biased. Further analysis of this question is still underway. In particular, analysis is just beginning of a special attrition interview which was administered to as many of the dropout families as could be located near the end of the experiment. This interview sought to ascertain the reasons for attrition, as well as the subsequent labor force behavior of families who dropped out.

While it is impossible to say definitely whether attrition bias is likely to create an overestimate or an underestimate of labor supply response, one general observation is in order. One would not expect those families who responded to the experimental payments with large reductions in labor supply to drop out of the experiment, for to do so would mean giving up the very payments which allowed them to reduce their work effort. If this is the case, and dropout families tended to be those who responded least to the payments, the observed reduction in labor supply of treatment families relative to controls may be an overestimate. Direct confirmation of this hypothesis is difficult, however, because of the limited amount

of data available for drop-outs and the extreme variability of measures of labor supply for individual families over time.

Finally, it should be emphasized that the experimental plans included none of the non-financial components which have been widely proposed as adjuncts to cash transfers to the working poor. There was, for example, no work requirement, job training or placement, or attempt at job creation. The experimental treatments consisted solely of cash payments. To the extent that these other components might be included in a national program, the labor supply reduction it might induce could be expected to be less than that observed in the experiment. At the same time, the modest levels of labor supply reduction observed among families receiving experimental payments indicates that these work-inducing components may not be necessary or cost-effective, at least if they are intended only to prevent a reduction in work effort.

Other Behavioral Responses

While the primary focus of the experiment was on labor supply behavior, a great deal of information about other economic, social and psychological attributes was collected in the quarterly interviews. These data include measures of consumption patterns, health and health care utilization, psychological factors such as self-esteem and perceived quality of life, social integration, leisure activity, life style enhancement, fertility, household composition, and marital stability.

In the analysis of the experimental results, these measures have served two distinct functions. First, where appropriate, they have been used as control variables in the anlysis of labor supply response. In this capacity,

they both improve the precision of estimation of the experimental effects on labor supply and also yield findings that are themselves of policy interest. For example, inclusion of measures of physical health indicates that the labor supply reduction of husbands in response to the experimental treatments was significantly stronger among individuals in poor health than among healthier workers.

Second, these measures have also been analyzed to detect any significant experimental impacts in areas other than labor supply. A priori, it might be expected that cash transfers would affect at least some of these characteristics. Abstracts of studies which analyzed a large number of such response variables are presented in Appendix C; for the sake of brevity, these results will not be discussed in detail here.* In general, however, few significant experimental impacts were found in non-economic areas. Where statistically significant effects were detected, they seldom formed any consistent pattern.

The general absence of experimental effects in non-economic areas may reflect several factors. It is true that many of the variables analyzed reflect basic personal attributes which change only slowly in response to external circumstances. Thus, it is possible that in the long run a permanent national transfer program might have effects which are not discernable in a three-year experiment. It is also possible, however, that cash payments of several hundred, or even several thousand, dollars per year do not alter the life circumstances of low-income families substantially enough to cause significant changes in basic personal attributes, even in the long run.

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* These studies are contained in Part D.I-VIII of the Technical Papers.

Further Research

It would be a misnomer to term the results reported here and in the Technical Papers the "final report" of the experiment. These results do represent a relatively comprehensive analysis of the major issues posed at the outset of the experiment, and it is unlikely that they will be substantially altered by subsequent analysis. But the data base produced by the experiment is an exceedingly rich and versatile one, and there are still many unresolved issues to be pursued -- indeed, many new questions have been raised by the present analysis. In a real sense, then, publication of these results marks only the beginning of a research effort which is likely to continue for years.

To facilitate this effort, the experimental data are now being made available to the research community at large. The Institute for Research on Poverty will furnish, at cost, fully documented computer tapes containing any subset of the data desired by researchers. In this way, it is hoped that maximum use will be made of this unique data set.*

The Poverty Institute itself is continuing its analysis of the experimental outcomes. As noted above, further research is being devoted to the effects of sample attrition and the labor supply response of families other than the continuous husband-wife families analyzed here. In addition, the behavior of several population groups

* A brief description of the data is included in Appendix D. Inquiries with respect to the data may be addressed to the Institute for Research on Poverty, University of Wisconsin, Madison, Wisconsin.

within the sample will be more intensively analyzed; these include the black control families; white families in Scranton, as compared with other white families in the sample; and welfare families. More detailed analysis of the separate effects of basic benefit levels and implicit tax rates is also underway. Other studies will focus on the dynamics of labor supply response, school performance of children, health and health care, and a variety of social and psychological variables.

These and subsequent research results from the experiment will, of course, be subjected to the critical review of the larger research community. That review will undoubtedly suggest additional areas of investigation and alternative analytical models to be tested. A conference of experts in social experimentation and labor economics will be convened under the auspices of the Brookings Institution in the Spring of 1974 to review the experimental research methodology and results. It is also anticipated that technical papers summarizing the labor supply analysis will be published in the Spring, 1974, issue of the Journal of Human Resources, and individual studies will be published in other professional journals.

The results of this experiment will be augmented and extended as data become available from the other income maintenance experiments now in progress. Analysis is now underway of the Iowa-North Carolina experiment sponsored by the Office of Economic Opportunity as a rural counterpart to the New Jersey-Pennsylvania urban experiment. Results from this project are expected within the next six months.

In addition, the Department of Health, Education, and Welfare is currently conducting similar experiments in Denver, Colorado; Seattle, Washington; and Gary, Indiana. These projects are still in the operational phase and will not be completed for several years.

# Appendix A:

# DESCRIPTIVE TABLES

DESCRIPTIVE TABLES

1. Sample Allocation, by plan, site, ethnic group, and income level - total sample

2. Sample Allocation by plan, site, ethnic group, and income level - continuous husband-wife sample (H/W)

3. Average Weekly Payment Level per Family, by ethnic group and plan (H/W)

4. Average Weekly Family Earnings, by ethnic group and plan (H/W)

5. Average Weekly Earnings of Husband,by ethnic group and plan (H/W)

6. Average Weekly Earnings of Wife, by ethnic group and plan (H/W)

7. Average Weekly Hours Worked per Family, by ethnic group and plan (H/W)

8. Average Weekly Hours Worked by Husband,by ethnic group and plan (H/W)

9. Average Weekly Hours Worked by Wife, by ethnic group and plan (H/W)

10. Average Number of Employed Persons per Family, by ethnic group and plan (H/W)

11. Employment Rate for Husbands, by ethnic group and plan (H/W)

12. Employment Rate for Wives, by ethnic group and plan (H/W)

13. Percentage Change in Average Weekly Payment Level per Family, first quarter to quarters 10 & 12, by ethnic group and plan (H/W)

14. Percentage Change in Average Weekly Earnings Per Family, first quarter to quarters 10 & 12, by ethnic group and plan (H/W)

15. Percentage Change in Average Weekly Earnings of Husband, first quarter to quarters 10 & 12, by ethnic group and plan (H/W)

16. Percentage Change in Average Weekly Earnings of Wife, first quarter to quarters 10 & 12, by ethnic group and plan (H/W)

17. Percentage Change in Average Weekly Hours Worked per Family, first quarter to quarters 10 & 12, by ethnic group and plan (H/W)

18. Percentage Change in Average Weekly Hours Worked by Husband, first quarter to quarter 10 & 12, by ethnic group and plan (H/W)

19. Percentage Change in Average Weekly Hours Worked by Wife, first quarter to quarters 10 & 12, by ethnic group and plan (H/W)

TABLE 1

Sample Allocation - Total Sample*

| | TOTAL | WHITE | BLACK | SPANISH-SPEAKING AMERICANS |
|---|---|---|---|---|
| TOTAL | 1357 | 440 | 512 | 415 |
| NIT PLAN: | | | | |
| 50-30 | 48 (3.5) | 19 (4.3) | 19 (3.8) | 10 (2.4) |
| 50-50 | 73 (5.4) | 15 (3.4) | 28 (5.6) | 30 (7.2) |
| 75-30 | 100 (7.4) | 26 (5.9) | 41 (8.1) | 34 (8.2) |
| 75-50 | 117 (8.6) | 33 (7.5) | 43 (8.6) | 41 (9.9) |
| 75-70 | 85 (6.3) | 31 (7.0) | 38 (7.6) | 16 (3.9) |
| 100-50 | 77 (5.7) | 22 (5.0) | 32 (6.4) | 23 (5.5) |
| 100-70 | 86 (6.3) | 25 (5.7) | 34 (6.8) | 27 (6.5) |
| 125-50 | 138(10.2) | 61(13.9) | 47 (9.4) | 30 (7.2) |
| Controls | 632(46.6) | 208(47.2) | 220(43.8) | 204(49.2) |
| SITE: | | | | |
| Trenton | 159(11.7) | 25 (5.7) | 105(20.9) | 29 (7.0) |
| Paterson-Passaic | 490(36.1) | 49(11.1) | 194(38.6) | 247(59.5) |
| Jersey City | 390(28.7) | 52(11.8) | 199(39.6) | 139(33.5) |
| Scranton | 318(23.4) | 314(71.4) | 4 (0.8) | 0 (0) |
| PRE-EXPERIMENT INCOME STRATUM (percent of poverty line): | | | | |
| 0- 99 | 414(30.5) | 119(27.0) | 139(27.7) | 156(37.6) |
| 100-124 | 454(33.5) | 153(34.8) | 173(34.5) | 128(30.8) |
| 125-150 | 489(36.0) | 168(38.2) | 190(37.8) | 131(31.6) |

* The first entries in each column are the number of families; the second entries (in parenthesis) are the percent of total families.

TABLE 2

Sample Allocation - Continuous Husband-Wife Sample*

| | TOTAL | WHITE | BLACK | SPANISH-SPEAKING AMERICANS |
|---|---|---|---|---|
| TOTAL | 693 | 310 | 234 | 149 |
| NIT PLAN: | | | | |
| 50-30 | 27 (3.9) | 13 (4.2) | 8 (3.4) | 6 (4.0) |
| 50-50 | 32 (4.6) | 11 (3.5) | 12 (5.1) | 9 (6.0) |
| 75-30 | 60 (8.7) | 22 (7.0) | 23 (9.8) | 15(10.1) |
| 75-50 | 65 (9.4) | 24 (7.7) | 25(10.7) | 16(10.7) |
| 75-70 | 48 (6.9) | 24 (7.7) | 21 (9.0) | 3 (2.0) |
| 100-50 | 44 (6.3) | 20 (6.5) | 14 (6.0) | 10 (6.7) |
| 100-70 | 53 (7.6) | 21 (6.8) | 17 (7.3) | 15(10.1) |
| 125-50 | 96(13.9) | 46(14.8) | 31(13.2) | 19(12.8) |
| Controls | 268(38.7) | 129(41.6) | 83(35.5) | 56(37.6) |
| SITE: | | | | |
| Trenton | 60 (8.7) | 12 (3.9) | 38(16.2) | 10 (6.7) |
| Paterson-Passaic | 158(22.8) | 30 (9.7) | 59(25.2) | 69(46.3) |
| Jersey City | 236(34.0) | 32(10.3) | 134(57.3) | 70(47.0) |
| Scranton | 239(34.5) | 236(76.1) | 3 (1.3) | 0 (0) |
| PRE-EXPERIMENT INCOME STRATUM (percent of poverty line) | | | | |
| 0- 99 | 179(25.8) | 71(22.9) | 53(22.6) | 55(36.9) |
| 100-124 | 237(34.2) | 105(33.9) | 85(36.3) | 47(31.5) |
| 125-150 | 277(40.0) | 134(43.2) | 96(41.0) | 47(31.5) |

* The first entries in each column are the numbers of families; the second entries (in parenthesis) are the percent of total families.

TABLE 3

Average Weekly Payment Level per Family - Continuous Husband-Wife Sample

| | 1st Year | 2nd Year | 3rd Year |
|---|---|---|---|
| TOTAL SAMPLE: | | | |
| Experimentals | $23 | $23 | $24 |
| Controls | 0 | 0 | 0 |
| WHITES: | | | |
| Experimentals | 22 | 23 | 23 |
| Controls | 0 | 0 | 0 |
| BLACKS: | | | |
| Experimentals | 24 | 24 | 25 |
| Controls | 0 | 0 | 0 |
| SPANISH-SPEAKING AMERICANS: | | | |
| Experimentals | 22 | 23 | 24 |
| Controls | 0 | 0 | 0 |
| NIT PLAN: | | | |
| 50-30 | 12 | 12 | 11 |
| 50-50 | 5 | 5 | 5 |
| 75-30 | 27 | 26 | 25 |
| 75-50 | 12 | 10 | 10 |
| 75-70 | 7 | 7 | 7 |
| 100-50 | 30 | 31 | 33 |
| 100-70 | 13 | 15 | 15 |
| 125-50 | 46 | 48 | 51 |

TABLE 4

Average Weekly Earnings per Family - Continuous Husband-Wife Sample

| | PRE-ENROLLMENT | 1st Year | 2nd Year | 3rd Year |
|---|---|---|---|---|
| TOTAL SAMPLE: | | | | |
| Experimentals | 95 | 108 | 114 | 124 |
| Controls | 95 | 105 | 113 | 128 |
| WHITES: | | | | |
| Experimentals | 100 | 107 | 114 | 126 |
| Controls | 98 | 107 | 122 | 143 |
| BLACKS: | | | | |
| Experimentals | 94 | 115 | 120 | 128 |
| Controls | 90 | 105 | 104 | 110 |
| SPANISH-SPEAKING AMERICANS: | | | | |
| Experimentals | 87 | 99 | 101 | 112 |
| Controls | 94 | 101 | 106 | 119 |
| NIT PLAN: | | | | |
| 50-30 | 92 | 99 | 99 | 93 |
| 50-50 | 96 | 113 | 125 | 146 |
| 75-30 | 93 | 107 | 118 | 125 |
| 75-50 | 91 | 112 | 123 | 135 |
| 75-70 | 94 | 114 | 112 | 124 |
| 100-50 | 95 | 103 | 102 | 106 |
| 100-70 | 98 | 111 | 112 | 130 |
| 125-50 | 99 | 105 | 112 | 122 |

TABLE 5

Average Weekly Earnings of Husband - Continuous Husband-Wife Sample

| | PRE-ENROLLMENT | 1st Year | 2nd Year | 3rd Year |
|---|---|---|---|---|
| TOTAL SAMPLE: | | | | |
| Experimentals | 85 | 97 | 100 | 106 |
| Controls | 84 | 89 | 95 | 103 |
| WHITES: | | | | |
| Experimentals | 88 | 96 | 100 | 106 |
| Controls | 90 | 92 | 101 | 111 |
| BLACKS: | | | | |
| Experimentals | 84 | 102 | 105 | 108 |
| Controls | 75 | 85 | 86 | 90 |
| SPANISH-SPEAKING AMERICANS: | | | | |
| Experimentals | 80 | 90 | 93 | 104 |
| Controls | 83 | 90 | 96 | 102 |
| NIT PLAN: | | | | |
| 50-30 | 76 | 83 | 88 | 77 |
| 50-50 | 85 | 99 | 109 | 125 |
| 75-30 | 82 | 98 | 104 | 110 |
| 75-50 | 86 | 103 | 106 | 111 |
| 75-70 | 80 | 97 | 96 | 102 |
| 100-50 | 81 | 89 | 91 | 92 |
| 100-70 | 90 | 99 | 100 | 109 |
| 125-50 | 89 | 96 | 101 | 110 |

TABLE 6

Average Weekly Earnings of Wife - Continuous Husband-Wife Sample

| | PRE-ENROLLMENT | 1st Year | 2nd Year | 3rd Year |
|---|---|---|---|---|
| TOTAL SAMPLE: | | | | |
| Experimentals | 7 | 7 | 8 | 10 |
| Controls | 6 | 8 | 7 | 10 |
| WHITES: | | | | |
| Experimentals | 7 | 6 | 7 | 9 |
| Controls | 3 | 7 | 8 | 10 |
| BLACKS: | | | | |
| Experimentals | 8 | 11 | 11 | 14 |
| Controls | 8 | 10 | 7 | 12 |
| SPANISH-SPEAKING AMERICANS: | | | | |
| Experimentals | 5 | 5 | 3 | 5 |
| Controls | 8 | 7 | 6 | 8 |
| NIT PLAN: | | | | |
| 50-30 | 11 | 11 | 7 | 8 |
| 50-50 | 10 | 10 | 12 | 15 |
| 75-30 | 7 | 5 | 8 | 8 |
| 75-50 | 3 | 5 | 9 | 11 |
| 75-70 | 8 | 12 | 10 | 14 |
| 100-50 | 8 | 10 | 9 | 8 |
| 100-70 | 4 | 7 | 6 | 11 |
| 125-50 | 6 | 4 | 4 | 6 |

TABLE 7

Average Weekly Hours Worked per Family - Continuous Husband-Wife Sample

| | PRE-ENROLLMENT | 1st Year | 2nd Year | 3rd Year |
|---|---|---|---|---|
| TOTAL SAMPLE: | | | | |
| Experimentals | 41 | 40 | 39 | 41 |
| Controls | 41 | 43 | 42 | 45 |
| WHITES: | | | | |
| Experimentals | 42 | 40 | 40 | 42 |
| Controls | 42 | 44 | 46 | 51 |
| BLACKS: | | | | |
| Experimentals | 40 | 42 | 40 | 41 |
| Controls | 36 | 41 | 39 | 37 |
| SPANISH-SPEAKING AMERICANS: | | | | |
| Experimentals | 38 | 37 | 37 | 38 |
| Controls | 44 | 42 | 41 | 42 |
| NIT PLAN: | | | | |
| 50-30 | 41 | 42 | 39 | 36 |
| 50-50 | 42 | 44 | 42 | 46 |
| 75-30 | 40 | 39 | 40 | 39 |
| 75-50 | 36 | 39 | 40 | 43 |
| 75-70 | 42 | 41 | 38 | 39 |
| 100-50 | 39 | 38 | 35 | 35 |
| 100-70 | 43 | 41 | 39 | 45 |
| 125-50 | 41 | 39 | 39 | 41 |

TABLE 8

Average Weekly Hours Worked by Husband - Continuous Husband-Wife Sample

| | PRE-ENROLLMENT | 1st Year | 2nd Year | 3rd Year |
|---|---|---|---|---|
| TOTAL SAMPLE: | | | | |
| Experimentals | 35 | 34 | 33 | 33 |
| Controls | 35 | 34 | 33 | 34 |
| WHITES: | | | | |
| Experimentals | 35 | 34 | 32 | 32 |
| Controls | 37 | 35 | 35 | 36 |
| BLACKS: | | | | |
| Experimentals | 34 | 35 | 33 | 32 |
| Controls | 30 | 31 | 30 | 29 |
| SPANISH-SPEAKING AMERICANS: | | | | |
| Experimentals | 34 | 33 | 33 | 34 |
| Controls | 38 | 37 | 35 | 35 |
| NIT PLAN: | | | | |
| 50-30 | 32 | 33 | 32 | 28 |
| 50-50 | 37 | 37 | 34 | 36 |
| 75-30 | 34 | 35 | 34 | 33 |
| 75-50 | 33 | 34 | 33 | 33 |
| 75-70 | 33 | 33 | 30 | 30 |
| 100-50 | 32 | 32 | 29 | 29 |
| 100-70 | 38 | 35 | 33 | 34 |
| 125-50 | 35 | 34 | 34 | 34 |

TABLE 9

Average Weekly Hours Worked by Wife - Continuous Husband-Wife Sample

| | PRE-ENROLLMENT | 1st Year | 2nd Year | 3rd Year |
|---|---|---|---|---|
| TOTAL SAMPLE: | | | | |
| Experimentals | 7 | 3 | 4 | 4 |
| Controls | 3 | 4 | 4 | 5 |
| WHITES: | | | | |
| Experimentals | 4 | 3 | 3 | 4 |
| Controls | 2 | 4 | 4 | 5 |
| BLACKS: | | | | |
| Experimentals | 4 | 5 | 5 | 6 |
| Controls | 3 | 5 | 4 | 5 |
| SPANISH-SPEAKING AMERICANS: | | | | |
| Experimentals | 3 | 2 | 1 | 2 |
| Controls | 5 | 4 | 3 | 4 |
| NIT PLAN: | | | | |
| 50-30 | 6 | 6 | 3 | 4 |
| 50-50 | 4 | 5 | 5 | 6 |
| 75-30 | 4 | 2 | 3 | 3 |
| 75-50 | 2 | 2 | 4 | 4 |
| 75-70 | 5 | 6 | 5 | 5 |
| 100-50 | 5 | 5 | 4 | 3 |
| 100-70 | 3 | 3 | 3 | 5 |
| 125-50 | 3 | 2 | 2 | 3 |

TABLE 10

Average Number of Employed Persons per Family - Continuous Husband-Wife Sample

| | PRE-ENROLLMENT | 1st Year | 2nd Year | 3rd Year |
|---|---|---|---|---|
| TOTAL SAMPLE: | | | | |
| Experimentals | 1.07 | 1.07 | 1.03 | 1.07 |
| Controls | 1.04 | 1.16 | 1.15 | 1.19 |
| WHITES: | | | | |
| Experimentals | 1.10 | 1.07 | 1.07 | 1.14 |
| Controls | 1.05 | 1.17 | 1.26 | 1.37 |
| BLACKS: | | | | |
| Experimentals | 1.05 | 1.10 | 1.04 | 1.04 |
| Controls | .98 | 1.16 | 1.07 | .98 |
| SPANISH-SPEAKING AMERICANS: | | | | |
| Experimentals | 1.03 | 1.00 | .94 | .98 |
| Controls | 1.13 | 1.13 | 1.04 | 1.08 |
| NIT PLAN: | | | | |
| 50-30 | 1.07 | 1.14 | 1.08 | 1.01 |
| 50-50 | 1.06 | 1.14 | 1.07 | 1.13 |
| 75-30 | 1.05 | 1.06 | 1.08 | 1.03 |
| 75-50 | .94 | 1.01 | 1.05 | 1.15 |
| 75-70 | 1.17 | 1.10 | 1.03 | 1.05 |
| 100-50 | 1.11 | 1.03 | .91 | .90 |
| 100-70 | 1.09 | 1.07 | 1.05 | 1.20 |
| 125-50 | 1.09 | 1.05 | 1.02 | 1.05 |

TABLE 11

Employment Rate for Husbands - Continuous Husband-Wife Sample

| | PRE-ENROLMENT | 1st Year | 2nd Year | 3rd Year |
|---|---|---|---|---|
| TOTAL SAMPLE: | | | | |
| Experimentals | .89 | .89 | .86 | .84 |
| Controls | .88 | .88 | .88 | .85 |
| WHITES: | | | | |
| Experimentals | .91 | .88 | .86 | .83 |
| Controls | .91 | .88 | .89 | .88 |
| BLACKS: | | | | |
| Experimentals | .86 | .91 | .88 | .83 |
| Controls | .78 | .83 | .84 | .77 |
| SPANISH-SPEAKING AMERICANS: | | | | |
| Experimentals | .89 | .89 | .85 | .87 |
| Controls | .96 | .96 | .91 | .91 |
| NIT PLAN: | | | | |
| 50-30 | .85 | .85 | .84 | .78 |
| 50-50 | .87 | .91 | .86 | .87 |
| 75-30 | .83 | .94 | .94 | .86 |
| 75-50 | .85 | .91 | .85 | .86 |
| 75-70 | .87 | .85 | .83 | .76 |
| 100-50 | .86 | .84 | .79 | .77 |
| 100-70 | .96 | .90 | .86 | .87 |
| 125-50 | .94 | .91 | .88 | .85 |

TABLE 12

Employment Rate for Wives - Continuous Husband-Wife Sample

| | PRE-EMPLOYMENT | 1st Year | 2nd Year | 3rd Year |
|---|---|---|---|---|
| TOTAL SAMPLE: | | | | |
| Experimentals | .13 | .12 | .12 | .15 |
| Controls | .10 | .14 | .13 | .16 |
| WHITES: | | | | |
| Experimentals | .14 | .11 | .11 | .15 |
| Controls | .06 | .14 | .15 | .18 |
| BLACKS: | | | | |
| Experimentals | .15 | .17 | .16 | .19 |
| Controls | .12 | .18 | .13 | .16 |
| SPANISH-SPEAKING AMERICANS: | | | | |
| Experimentals | .10 | .08 | .05 | .08 |
| Controls | .14 | .11 | .09 | .10 |
| NIT PLAN: | | | | |
| 50-30 | .15 | .18 | .13 | .18 |
| 50-50 | .19 | .18 | .16 | .19 |
| 75-30 | .13 | .10 | .10 | .11 |
| 75-50 | .05 | .08 | .11 | .15 |
| 75-70 | .21 | .20 | .16 | .20 |
| 100-50 | .18 | .17 | .14 | .12 |
| 100-70 | .13 | .13 | .13 | .16 |
| 125-50 | .11 | .07 | .08 | .11 |

TABLE 13

Percentage Change in Average Weekly Payment Level per Family - Continuous Husband-Wife Sample

| | First Quarter Level | Percentage Change* Q1-Q10 | Q1-Q12 |
|---|---|---|---|
| TOTAL SAMPLE: | | | |
| Experimentals | $23 | 3.8 | 5.0 |
| Controls | 0 | 0 | 0 |
| WHITES: | | | |
| Experimentals | 22 | 2.3 | 3.0 |
| Controls | 0 | 0 | 0 |
| BLACKS: | | | |
| Experimentals | 24 | 0.7 | 1.7 |
| Controls | 0 | 0 | 0 |
| SPANISH-SPEAKING AMERICANS: | | | |
| Experimentals | 21 | 12.7 | 15.2 |
| Controls | 0 | 0 | 0 |
| NIT PLAN: | | | |
| 50-30 | 12 | -13.7 | - .6 |
| 50-50 | 3 | 54.1 | 70.2 |
| 75-30 | 27 | - 9.0 | - 8.6 |
| 75-50 | 13 | -22.1 | -28.2 |
| 75-70 | 7 | - 1.0 | 5.3 |
| 100-50 | 28 | 18.6 | 20.5 |
| 100-70 | 15 | - 3.6 | 9.4 |
| 125-50 | 46 | 11.3 | 10.2 |

* Q1, Q10, Q12 denote quarters 1, 10, and 12, respectively.

TABLE 14

Percentage Change in Average Weekly Earnings Per Family - Continuous Husband-Wife Sample

| | First Quarter Level | Percentage Change* Q1-Q10 | Q1-Q12 |
|---|---|---|---|
| TOTAL SAMPLE: | | | |
| Experimentals | $95 | 24.9 | 36.6 |
| Controls | 94 | 28.7 | 47.0 |
| WHITES: | | | |
| Experimentals | 100 | 17.3 | 39.4 |
| Controls | 98 | 36.8 | 63.6 |
| BLACKS: | | | |
| Experimentals | 94 | 32.9 | 36.9 |
| Controls | 90 | 24.3 | 23.7 |
| SPANISH-SPEAKING AMERICANS: | | | |
| Experimentals | 87 | 27.8 | 30.0 |
| Controls | 94 | 15.7 | 39.9 |
| NIT PLAN: | | | |
| 50-30 | 92 | -15.0 | 10.6 |
| 50-50 | 96 | 63.8 | 53.6 |
| 75-30 | 93 | 29.3 | 41.6 |
| 75-50 | 91 | 44.3 | 56.6 |
| 75-70 | 94 | 16.1 | 47.2 |
| 100-50 | 95 | 3.9 | 21.9 |
| 100-70 | 98 | 26.0 | 39.0 |
| 125-50 | 99 | 20.9 | 22.9 |

* Q1, Q10, Q12 denote quarters 1, 10, and 12, respectively.

TABLE 15

Percentage Change in Average Weekly Earnings of Husbands - Continuous Husband-Wife Sample

| | First Quarter Level | Percentage Change* Q1-Q10 | Q1 - Q12 |
|---|---|---|---|
| TOTAL SAMPLE: | | | |
| Experimentals | 85 | 21.4 | 30.0 |
| Controls | 84 | 16.0 | 31.7 |
| WHITES: | | | |
| Experimentals | 88 | 12.3 | 29.6 |
| Controls | 90 | 16.4 | 35.5 |
| BLACKS: | | | |
| Experimentals | 84 | 27.0 | 30.6 |
| Controls | 75 | 17.5 | 23.0 |
| SPANISH-SPEAKING AMERICANS: | | | |
| Experimentals | 80 | 31.6 | 29.7 |
| Controls | 83 | 13.2 | 34.0 |
| NIT PLAN: | | | |
| 50-30 | 76 | -16.1 | 7.4 |
| 50-50 | 85 | 62.1 | 52.3 |
| 75-30 | 82 | 28.1 | 40.1 |
| 75-50 | 86 | 28.1 | 38.1 |
| 75-70 | 80 | 16.5 | 40.7 |
| 100-50 | 81 | 11.0 | 19.5 |
| 100-70 | 90 | 11.6 | 21.3 |
| 125-50 | 89 | 21.4 | 21.6 |

* Q1, Q10, Q12 denote quarters 1, 10, and 12, respectively.

TABLE 16

Percentage Change in Average Weekly Earnings of Wife - Continuous Husband-Wife Sample

| | First Quarter Level | Percentage Change* Q1-Q10 | Q1-Q12 |
|---|---|---|---|
| TOTAL SAMPLE: | | | |
| Experimentals | 7 | 54.0 | 39.2 |
| Controls | 5 | 104.3 | 84.8 |
| WHITES: | | | |
| Experimentals | 7 | 41.8 | 32.3 |
| Controls | 3 | 284.8 | 282.8 |
| BLACKS: | | | |
| Experimentals | 8 | 76.9 | 73.7 |
| Controls | 8 | 76.8 | 48.6 |
| SPANISH-SPEAKING AMERICANS: | | | |
| Experimentals | 5 | 27.3 | - 6.3 |
| Controls | 8 | 9.4 | -10.0 |
| NIT PLAN: | | | |
| 50-30 | 11 | - 3.4 | -59.3 |
| 50-50 | 10 | 60.6 | 34.9 |
| 75-30 | 7 | 15.3 | 21.9 |
| 75-50 | 3 | 248.5 | 187.6 |
| 75-70 | 8 | 68.1 | 101.0 |
| 100-50 | 8 | 2.4 | 25.9 |
| 100-70 | 4 | 125.1 | 193.2 |
| 125-50 | 6 | 26.4 | 19.1 |

* Q1, Q10, Q12 denote quarters 1, 10, and 12, respectively.

TABLE 17

Percentage Change in Average Weekly Hours Worked per Family - Continuous Husband-Wife Sample

| | First Quarter Level | Percentage Change* | |
|---|---|---|---|
| | | Q1-Q10 | Q1-Q12 |
| TOTAL SAMPLE: | | | |
| Experimentals | 41 | - 4.3 | 3.7 |
| Controls | 41 | 6.6 | 17.1 |
| WHITES: | | | |
| Experimentals | 42 | - 7.4 | 7.5 |
| Controls | 42 | 14.5 | 32.1 |
| BLACKS: | | | |
| Experimentals | 40 | - 3.0 | .1 |
| Controls | 36 | 5.8 | 1.5 |
| SPANISH-SPEAKING AMERICANS: | | | |
| Experimentals | 38 | .2 | 1.4 |
| Controls | 44 | - 9.7 | 2.8 |
| NIT PLAN: | | | |
| 50-50 | 41 | -21.9 | - 5.2 |
| 50-50 | 42 | 13.9 | 10.9 |
| 75-30 | 40 | - 6.2 | - 5.0 |
| 75-50 | 36 | 11.7 | 24.5 |
| 75-70 | 42 | -20.0 | 2.1 |
| 100-50 | 39 | 18.1 | - 4.8 |
| 100-70 | 43 | 1.5 | 7.0 |
| 125-50 | 41 | - 3.2 | - .9 |

* Q1, Q10, Q12 denote quarters 1, 10, and 12, respectively.

TABLE 18

Percentage Change in Average Weekly Hours Worked by Husband - Continuous Husband-Wife Sample

| | First Quarter Level | Percentage Change* Q1-Q10 | Q1-Q12 |
|---|---|---|---|
| TOTAL SAMPLE: | | | |
| Experimentals | 35 | - 9.5 | - 4.0 |
| Controls | 35 | 7.0 | - .3 |
| WHITES: | | | |
| Experimentals | 35 | -15.2 | - 4.4 |
| Controls | 37 | - 5.0 | 2.6 |
| BLACKS: | | | |
| Experimentals | 34 | - 8.9 | - 5.5 |
| Controls | 30 | - 5.7 | - 3.3 |
| SPANISH-SPEAKING AMERICANS: | | | |
| Experimentals | 34 | 3.1 | - .7 |
| Controls | 38 | -13.2 | - 3.4 |
| NIT PLAN: | | | |
| 50-30 | 32 | -24.5 | - 9.4 |
| 50-50 | 37 | 5.6 | .5 |
| 75-30 | 34 | - 7.4 | - 5.3 |
| 75-50 | 33 | - 4.9 | 6.7 |
| 75-70 | 33 | -16.8 | 1.5 |
| 100-50 | 32 | -10.2 | - 7.1 |
| 100-70 | 38 | -16.2 | -16.0 |
| 125-50 | 35 | - 7.5 | - 4.3 |

* Q1, Q10, Q12, denote quarters 1, 10, and 12, respectively.

TABLE 19

Percentage Change in Average Weekly Hours Worked by Wife - Continuous Husband-Wife Sample

| | First Quarter Level | Percentage Change* Q1-Q10 | Q1-Q12 |
|---|---|---|---|
| TOTAL SAMPLE: | | | |
| Experimentals | 4 | 19.8 | - 4.0 |
| Controls | 3 | 87.9 | 64.1 |
| WHITES: | | | |
| Experimentals | 4 | 20.0 | -10.4 |
| Controls | 1 | 219.8 | 215.0 |
| BLACKS: | | | |
| Experimentals | 4 | 28.8 | 8.3 |
| Controls | 3 | 92.2 | 53.2 |
| SPANISH-SPEAKING AMERICANS: | | | |
| Experimentals | 3 | - 3.4 | -18.8 |
| Controls | 5 | -12.4 | 33.1 |
| NIT PLAN: | | | |
| 50-30 | 6 | - 9.9 | -59.2 |
| 50-50 | 4 | 39.6 | 11.1 |
| 75-30 | 4 | -21.4 | 36.3 |
| 75-50 | 2 | 141.9 | 91.9 |
| 75-70 | 5 | 4.6 | 3.8 |
| 100-50 | 5 | 31.9 | -39.9 |
| 100-70 | 3 | 89.9 | 108.8 |
| 125-50 | 3 | 23.6 | 27.3 |

*Q1, Q10, Q12 denote quarters 1, 10, and 12, respectively.

## Appendix B:

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Chapter III
(a) The Labor Supply Response of Married Women, Glen Cain et al.
(b) Relationship of the Female Labor Supply Characteristics of the Experimental Sample to Those of Other Samples, Walter Nicholson

Chapter IV — School Enrollment and Labor-Force Participation Among Young Adults, Charles Mallar

Chapter V
(a) Labor Supply of the Family, Robinson Hollister
(b) The Effects of the Welfare "Bias" on Family Earnings Response to the Experiment, Robert Avery

Chapter VI — Wage Rate Response, Harold W. Watts and John Mamer

Chapter VII — The Effect of Negative Income Tax Payments on Job Turnover and Job Selection, Seymour Spilerman and Richard Miller

Chapter VIII — The Effects of Health on the Supply of and Returns to Labor, David Elesh and Myron J. Lefcowitz

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Chapter II Measurement of the Experimental Variables and Related Measurement Issues, Harold W. Watts

Chapter III
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(b) Expenditure Patterns in the Graduated Work Incentive Experiment: A Descriptive Survey, Walter Nicholson
(c) Consumption Behavior Under a Permanent Negative Income Tax: Preliminary Evidence, Charles Metcalf

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Chapter VI Social Integration, Leisure Activity, Media Exposure, and Life-Style Enhancement, Jack Ladinsky and Anna Wells

Chapter VII The Effect of Income Maintenance Laws on Fertility, Glen Cain

Chapter VIII Changes in Household Composition, Jon H. Knudsen, Robert A. Scott, and Arnold R. Shore

* See Appendix C for abstracts of these papers.

PART E. TECHNICAL NOTES


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| Chapter II | Technical Note on Cubic Splines; Dale Poirier |
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## Appendix C:

ABSTRACTS OF RESEARCH INTO BEHAVIORAL RESPONSES TO THE EXPERIMENT OTHER THAN THE LABOR SUPPLY RESPONSE
(Part D of Technical Papers)

## ABSTRACTS OF RESEARCH INTO BEHAVIORAL RESPONSES TO THE EXPERIMENT OTHER THAN THE LABOR SUPPLY RESPONSE (Part D of Technical Papers)

Housing Consumption in the New Jersey-Pennsylvania Experiment (Judith Wooldridge). An important hypothesis often put forward is that a guaranteed income of some kind will enable low-income people to acquire better housing. This study, therefore, estimated (within the constraints of very inadequate data) the effects of the experiment on house buying and quality of rental housing. The primary finding was that it was increases in earned income, not payments, that played the major role in stimulating increased home ownership during the experiment. Experimental payments were more likely to be used for improved rental housing than for home buying. On the other hand, experimental families with enough income to be above their breakeven point (and therefore receiving no payments) were more likely to acquire homes than their control counterparts--one apparent explanation being that their guaranteed incomes gave them the financial security to do so.

Expenditure Patterns in the Graduated Work Incentive Experiment: A Descriptive Survey (Walter Nicholson). The purpose of this research was to provide a descriptive survey of the consumption data from the New Jersey-Pennsylvania experiment. For this reason, relatively simple statistical tests and regression specifications which resembled those typically used in cross-section studies of consumption behavior were used, which should indicate to future researchers possible relationships between the experimental data and other bodies of data. There are, however, limitations to such an approach which should be clearly recognized. The two most important are these: (1) This approach ignores the cross-substitution effects on consumption behavior which arise because the price of "leisure" differs systematically across experimental families and (2) it neglects the long-term nature of consumption decisions and does not incorporate long-term variables into the specification of the income series being used.

Within these constraints, two specific questions were addressed: (1) Did experimental families allocate their payment income differently from other increases in purchasing power? (2) To what extent were the expenditure patterns of homeowners and renters structurally distinct?

With respect to the first question, the evidence suggested that control and experimental families did not differ greatly in the ways in which they expended their incomes. Concerning the second issue, homeowners (and those moving from renter to owner status) behaved in substantially different ways from renters; specifically, homeowners were more likely to use payments for the purchase of durable goods. Structural differences by race were also suggested. White and Spanish-speaking families have higher marginal propensities to spend on food eaten at home than do black families. For Spanish-speaking families there is some evidence of important effects of experimental payments on durable acquisitions.

Consumption Behavior Under a Permanent Negative Income Tax: Preliminary Evidence (Charles Metcalf). This research analyzed durable goods purchases and asset and debt accumulations primarily from the perspective of providing insights into the impact of a permanently adapted negative income tax (serving as a companion paper to the one described above on Predicting the Effects of Permanent Programs from a Limited-Duration Experiment.) The empirical strategy differs from that adopted by Nicholson in two major respects. First, the model specification paralled more directly those adopted for estimating labor supply responses; thus, the level of earned income was regarded as an outcome of the labor supply decision rather than as an independent variable in a conventional system of expenditure equations. Second, stress was placed on interpreting variations in expenditure behavior over the span of the experiment; the most striking results were obtained for the first three months and for the second half of the experiment.

While the statistical power of many of the results was weak, evidence of both positive net saving (normally associated with temporary income changes) and of substitution from future to current consumption (associated with the temporary price change implicit in the experimental tax rate) was found. From the fragmentary information available, the marginal propensity to save out of experimental payments appeared to reach 21 percent during the second half of the experiment, and was positive for all ethnic groups. While it is sometimes argued that the behavior of low-income households cannot be interpreted within the context of conventional economic models such as those represented by the permanent income hypothesis and related theories, results obtained from the experiment have provided renewed optimism in this direction.

Experimental Effects on Health and Health Care Utilization (Myron J. Lefcowitz and David Elesh). The effect of the experimental treatments on investments in health and the utilization of health care for the head, spouse, and children separately was assessed for the three points in time during the experiment at which health data were gathered. Two measures of health status were used: a person's number of chronic illnesses and days lost from work (including housework or school) due to illness. The measures of health care utilization were the number of days spent in a hospital (for children, it was whether or not there had been a hospital stay), the total number of physician visits, the number of private physician visits, and the number of "other" physician visits. No evidence of any experimental effects was observed, either for the health measures or for the measures of the health-care utilization. This was not a surprising finding, given the fact that the effects of income on these variables have usually been found to be small or nonexistent.

Arguments that the failure to find any experimental effects is due to the experiment's limited duration, or to our sample's lack of access to care, or to meagerness of the experimental payments, are not borne out. While the experiment was limited to only three years, the fact that people generally put an extremely high priority on their health suggests that the effects--if they were to be observed at all--should have occurred quite rapidly. Moreover, the utter lack of any sign of an increase in illness or utilization over the time makes it unlikely that a longer experiment would produce different results. As far as the question of access to care is concerned, there is no evidence that our sample is seriously disadvantaged in this respect. The vast majority of the families report that they have a regular physician within a reasonable distance from their homes. Finally, the argument that the experimental payments are too small to have any effect can be dismissed with the observation that the average payment per family per year was about $1,000; since average family earnings for the whole sample were approximately $5,500, this amounts to 18 percent of their earnings--a non-trivial amount.

Social-Psychological Consequences of the Graduated Work Incentive Experiment ( Russell Middleton and Vernon Allen). This investigation was concerned with whether the experiment had an effect on a series of social-psychological variables among the male heads of families participating in the program. Scales were constructed to measure sense of community efficacy, support for government social programs, sense of control of future, anomy, self-esteem, and psychosomatic and nervous symptoms. In addition, five individual items assessed worry; five items from the Cantril scale measured present, past and expected quality of life; and other items dealt with future job, perceived financial well-being, amount of money needed, and behavior if given more money.

In most cases the analysis revealed no significant differences between the experiment and control groups in their social-psychological reactions, or among responses to the different plans. There were only scattered instances of significant differences, and these did not constitute a systematic or meaningful pattern. Furthermore, there were no systematic differences in the social-psychological reactions of major subgroups--such as different ethnic groups, different age groups, different education groups, and residents of different cities.

The possibility that positive psychological effects of the program payments might have been cancelled out by concomitant negative consequences of a possible reduction in hours worked or earned family income was then examined--the expectation being that any experimental effect on the social-psychological variables would be a consequence primarily of a family's receiving additional income through the program payments. Using path analysis, however, only five out of 48 instances were found where program payments were significantly related to the social-psychological variables. There was also little if any experimental effect on hours worked by the male head or on family earnings.

Finally, a canonical correlation procedure was employed to summarize the effects of the experiment on a broad range of social-psychological variables considered as indicators of "general psychological well-being". This analysis also showed little or no effect of the experiment.

It is important to note that little or no negative consequences, such as a loss of self-esteem, were found. A slight positive effect was expected on the basis of numerous studies which have shown that socioeconomic status and income are positively correlated with the variables that tap psychological well-being. The lack of positive results may have been due to a threshold effect; had the payments been appreciably greater or if the program had lasted for a longer period, perhaps there would have been an improvement on the part of the experimental subjects. Some of the data do suggest that the subjects did not perceive that their financial situation had been significantly improved relative to that of comparable others.

Social Integration, Leisure Activity, Media Exposure, and Life Style Enhancement (Jack Ladinsky and Anna Wells). The objectives of this research were to determine whether the experiment, especially during the second and third years, brought about perceptible changes of behavior in the following four areas: (1) family integration, social visiting, and aid-giving, (2) leisure pursuits, (3) mass media exposure, and (4) goods and services purchases for life-style enrichment. Underlying the research was a concern for evidence of changes in social integration or social cohesion--ties to family and friends, organization and community--as a result of the higher income and greater economic security for the experimental participants.

Within each of the four areas of concern, analysis was presented for one or more dependent variables using a basic regression model that varied only slightly from case to case. The independent variables in the regression model included background variables measured at pre-enrollment (or at some instances at first quarter), site variables, and variables relating to the experiment.

In the process of analyzing the four behavior areas, some statistically significant experimental effects were found (in six out of 42 regression models). But these effects were

sporadic. The experiment seemed to affect dependent variables that were unrelated in different ways (sometimes generally, other times through the guarantee or tax rate), and at only one point of measurement (when the dependent variables were measured at two or three points during the experiment). As a result, the effects did not seem substantively significant, and no meaningful interpretation could be given. Significant effects from independent variables, other than the experimental ones were invariably present, patterned, and more easily interpretable. For instance, education was positively related to newspaper reading at all three points in time. Thus, within the limits of our analysis, there was no evidence of an increase or decrease in social integration in the experimental group over the span of the experiment, and the control and experimental groups seemed very similar in their social behavior.

The Effect of Income Maintenance Laws on Fertility (Glen Cain). Because fertility decisions involve parental commitments for 18 years or so, the three-year experiment can offer only limited information on fertility responses to legislated income maintenance plans. During the three-year duration, however, the experimental plans changed economic variables affecting fertility by raising family incomes and by reducing child costs--both the direct costs and the opportunity costs (in terms of foregone earnings) of the wife's time. Despite these pro-natal changes, the statistical results for the first 10 quarters show no significant effect of the experimental plans on the rate of pregnancy and births. Moreover, there was no tendency among the treatment families for the more generous plans to be associated with higher fertility than the least generous plan.

The empirical results, taken at face value, say that urban poor families containing a husband and a wife of child-bearing age do not have more births when implicitly offered cash transfer payments for additional children during a three-year period. They do not, at least, when the cash payments range between $200 to $900 per year for the one or two years that they might actually receive such "baby bonuses." Moreover, these urban poor families do not have more births when confronted with relatively high implicit tax rates over the three-year period--rates which are believed to decrease market work and promote the substitution of homework and leisure.

Plans are underway to add the data from the final two quarters and to examine other measures of family planning behavior.

Changes in Household Composition (Jon Helge Knudsen, Robert A. Scott, Arnold R. Shore). The purpose of this research was to study, from a policy perspective, the impact of a negative income tax on household composition changes among families enrolled in the experiment. In the absence of firm theoretical propositions obtainable from the existing literature, transition probabilities for the sample were generated by means of a Markov chain analysis and then decomposed.

No evidence was found of changes in household composition among experimental families indicative of major disruptions in family life. Analysis of the changes that did occur focused on the impact of experimental parameters and selected policy-relevant variables. The findings were these: (1) Families on medium generosity plans had higher transition rates out of the nuclear status than those on high and low generosity plans. (2) Averaged over the whole sample, white families had lower rates of transition than either Spanish-speaking or black families. (3) Other variables such as husband's age, prior income, family size, and education had less impact on composition changes than ethnicity and plan. (4) Several interactions among variables were important, including the interactions between plan and ethnicity and between husband's age and ethnicity.

# APPENDIX D:

## DATA AVAILABLE FROM THE GRADUATED WORK INCENTIVE EXPERIMENT IN NEW JERSEY AND PENNSYLVANIA

# DATA AVAILABLE FROM THE GRADUATED WORK INCENTIVE EXPERIMENT IN NEW JERSEY AND PENNSYLVANIA

## 1. General Description of the Experiment and Available Data

The New Jersey-Pennsylvania Experiment was designed to measure the response of households containing an able-bodied male between 18 and 58 (not going to school full time, institutionalized, or in the armed forces) to a set of negative income tax plans. These plans guaranteed a certain payment (the guarantee) in the event that the household received no other income, and reduced this payment by a certain percentage (the tax rate) of every dollar earned up to the income level (breakeven level) at which the payment was reduced to zero. Four guarantee levels (50, 75, 100, 125 percent of the official poverty line) and three tax rates (30, 50, 70 percent were tested. These were combined into 8 separate negative income tax plans (50-30, 50-50, 75-30, 75-50, 75-70, 100-50, 100-70, 125-50). To be eligible, families had to have an income for the year preceding the experiment of not more than 150 percent of the poverty line.

Originally, 1 ?16 families were enrolled in the experiment: 725 in the experimental groups and 491 controls. They were enrolled sequentially at the following sites: Trenton, New Jersey (August, 1968); Paterson-Passaic, New Jersey (January, 1969); Jersey City, New Jersey (June, 1969); and Scranton, Pennsylvania (September, 1969). After the experiment was underway, 141 new controls were added in Trenton and Paterson-Passaic (October, 1969).

There are 7 categories of data available for analysis:

1. The Screening Interview

   This is a short interview designed to assess eligibility for inclusion in the experiment. The eligibility criteria were as follows:

   1) Family income had to be no higher than 150 percent of the official poverty line during the year preceding the experiment.
   2) The family had to include an able-bodied male, aged 18-58, who was not in the armed forces, institutionalized, or going to school full time, plus one other family member.

   The interview was administered to 28,000 families.

2. The Pre-enrollment Interview

   This interview was administered to collect extensive base-line data on all the families selected (from the screening interview) as eligible for inclusion in the experiment. It was administered to 2,341 families before enrollment.

3. Twelve Quarterly Interviews

   These interviews constituted the main data source for the experiment. They were administered to all families (1,357 were actually enrolled) 4 times a year for the three-year period over which payments were being made. They were approximately one hour long, and were composed of 2 sections: (1) A 20 minute section (called the "core") on the labor-force status

and participation of all family members 16 years of age or older. This section was repeated in every quarterly interview (i.e., every three months). (2) A non-core section which varied by quarter and covered at varying frequencies such items as assets, durable goods and debts, child care, child support, educational background and aspiration, family integration, leisure activity, vacations and hobbies, home ownership, insurance policies, job history, job satisfaction, media, medical and dental care, mobility, membership in organizations, political activity and involvement, social and demographic background, social-psychological attitudes, a time-budget study, and welfare status.

In addition to information obtained in direct responses to the questions on the quarterly interview, a group of constructed variables have been generated and added to the quarterly files. These are designed to provide standard information on labor-force participation, wages, and hours worked for all adult household members for the week previous to each quarterly interview. They were constructed using various sets and combinations of core variables, plus a conservative and strictly limited system of data editing and imputation.

4. 13 ("Follow-up") Quarterly Interview

This interview was administered three months after the transfer payments had ceased. It also contained a labor-force core section for each family member 16 or over. Its main purpose,

however, was to investigate the families' understanding of the experiment, and their reactions to the transfer payments and the interviews.

5. Special Attrition Interview

After the experiment was over a concerted effort was made to trace the 25 percent of families who had attrited during the experiment. About 41 percent of those families were found, and a special interview was administered to them. It included family composition changes since the last interview, plus a labor-force core, work history, job status, geographic mobility, and welfare status, since leaving the experiment. In addition, questions were asked to attempt to determine the reason for dropping out.

6. The Payments File

This file is composed of information from the income report forms required from families in the experimental group every 4 weeks in order to qualify for payments, and the actual amounts of each payment. The data collected include hours worked, gross earnings, certain deductions, and changes in family size. This file, of course, only exists for families in the experimental group, because control families submitted no Income Report Forms and received no payments.

7. The Annual Income Supplement

This interview administered at the end of every year of payments at each site. It was designed to get annual totals

for different categories of income for the purpose of getting an independent series for comparison with the monthly and quarterly income data collected on the income report forms and questionnaires, respectively.

The sections that follow list the specific data available.

## 2. QUARTERLY LABOR-FORCE DATA

### Core

Hours worked last week (month) - husband
- wife
- 1st other adult
- 2nd other adult
- 3rd other adult

Pay last week (month) - husband
- wife
- 1st other adult
- 2nd other adult
- 3rd other adult

Job - husband
- wife
- 1st other adult
- 2nd other adult
- 3rd other adult

Looking for work - husband
- wife
- 1st other adult
- 2nd other adult
- 3rd other adult

Other labor-force data - head
- spouse
- 1st other adult
- 2nd other adult
- 3rd other adult

### Constructed Variables

Earnings last week at all jobs - head
- spouse
- 1st other adult
- 2nd other adult
- 3rd other adult

Total household earnings last week

Total number of adult earners in household

Hours worked last week at all jobs -head
-spouse
-1st other adult
-2nd other adult
-3rd other adult

Regular hours worked last week on main job - head
- spouse
- 1st other adult
- 2nd other adult
- 3rd other adult

Total regular hours worked last week on main job by all adult in household

Total hours worked on main job last week - head
- spouse
- 1st other adult
- 2nd other adult
- 3rd other adult

Total hours worked on main job last week by all adults in household

Labor-force participation status last week - head
- spouse
- 1st other adult
- 2nd other adult
- 3rd other adult

Family labor-force participation status last week

Number of adults "available" for work*

Ratio of hours actually worked to total hours if all "available" adults worked 40 hours (realized work effort)

Ratio of hours actually worked to total feasible hours if all adults in household worked 40 hours (potential work effort)

Wage Rate - head
- spouse
- 1st other adult
- 2nd other adult
- 3rd other adult

Number of adults in household employed last week

Number of adults in household unemployed last week

Number of adults not in labor force or in labor force but further not classifiable

Number of adults with no labor force status classification

Guarantee level of experimental plan

Tax rate of experimental plan

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* excludes those in school, disabled, or women with children under 6.

Control or experimental status

Ethnicity of household head

Education of household head

Site

| 3. SUBJECT INDEX (excluding labor-force status) OF QUESTIONNAIRE CONTENTS | Specific Quarter Asked |
|---|---|
| AGE - head | |
| - spouse | |
| - children | |
| ATTITUDES (head of household) | |
| Anomie scale and fate control | Pre* |
| | 5th |
| | 7th |
| | 9th |
| Future events | 7th |
| | 11th |
| Outlook | |
| Happiness | Pre |
| | 5th |
| | 9th |
| Ladder of life | Pre |
| | 4th |
| | 8th |
| Worry Scale | Pre |
| | 5th |
| | 9th |
| Political | |
| Attitudes (Liberalism/Conservatism) | Pre |
| | 5th |
| | 9th |
| Awareness | Pre |
| | 1st |
| | 5th |
| | 9th |
| Integration | Pre |
| | 5th |
| | 9th |
| Self-esteem | 7th |
| | 11th |
| Social Desirability | 7th |
| Subjective Social Status | Pre |

*Pre-enrollment, baseline interview

| | Specific Quarter Asked |
|---|---|
| Time Horizon | 7th |
| Work | See job satisfaction |
| **EDUCATIONAL BACKGROUND AND SCHOOLING** | |
| Children | 1st<br>5th<br>9th |
| Children (Aspirations of parents for) | 1st<br>5th<br>9th |
| Husband | 1st |
| Wife | 1st |
| Husband's family-parents | 1st<br>11th |
| - male or female guardian | 11th |
| - siblings | 1st<br>11th |
| Wife's family-parents | 1st<br>11th |
| - siblings | 1st |
| **ETHNICITY - head** | |
| **FAMILY** | |
| Composition | Every quarter |
| Birth Control | 12th |
| Child Care-general | Every quarter |
| Child care-wife working or not working | 4th<br>8th<br>12th |
| Child Support and alimony | Every quarter<br>4th<br>11th |

| | Specific Quarter Asked |
|---|---|
| **Expenditure patterns** | |
| - appliances | 3rd<br>7th |
| - clothing | 3rd<br>7th<br>11th |
| - food | 3rd<br>7th<br>11th |
| **Family history and Background** | |
| - deceased parents of head | 11th |
| - marital history of head and spouse | 11th<br>12th |
| - marital status of head's parents | 11th |
| - religious background | 7th |
| - social & demographic background of head and spouse | 7th |
| - verbal facility of head and Ammons & Ammons Quick Test | 12th |
| **Family integration (general)** | Pre<br>3rd<br>5th<br>7th<br>9th |
| **Family integration (spouses separated)** | 11th<br>12th |
| **Homework and children's chores** | 1st<br>3rd<br>5th<br>7th<br>9th |
| **Leisure activity (family)** | 3rd<br>7th<br>11th |

| | Specific Quarter Asked |
|---|---|
| Leisure time spent with children | 3rd<br>7th<br>11th |
| Neighborhood - ethnic composition | 7th |
| Social integration | Pre<br>4th<br>8th<br>12th |
| Time budget study | 3rd |
| **FINANCIAL STATUS** | |
| Financial management | 1st<br>6th<br>10th |
| Credit and installment buying | 10th |
| Pawned items | 1st<br>6th<br>10th |
| Assets, savings | Pre<br>1st<br>6th<br>10th<br>12th |
| Debts | Pre<br>1st<br>6th<br>10th |
| Garnishments | 1st<br>6th<br>10th |
| Repossessions | 1st<br>6th<br>10th |
| Appliances owned | Pre<br>1st<br>6th<br>10th |

| | Specific Quarter Asked |
|---|---|
| Motor Vehicles owned | Pre<br>1st<br>6th<br>10th |
| Furniture owned | 1st<br>6th<br>10th |
| Insurance | |
| - general | Pre |
| - life | 4th<br>8th<br>12th |
| - medical | 2nd<br>6th |
| Income Tax Return (joint or separate) | 12th |
| HOME OWNERSHIP | |
| Housing & property ownership | AIS<br>Pre<br>4th |
| Family housing conditions | Pre |
| Home improvements | 3rd<br>7th<br>11th |
| JOB HISTORY AND EMPLOYMENT PATTERNS | |
| Head job history (lifetime) | 11th |
| Husband and wife job history-general | 2nd |
| Wife job history, current marriage (if working) | 4th<br>8th<br>12th |
| Wife job history, current marriage (if not working) | 4th<br>8th<br>12th |

| | Specific Quarter Asked |
|---|---|
| Occupational experience of head's parents or guardians | 11th |
| Occupational experience of head's siblings | 11th |
| Recent Work Experience (last week or last month) each adult | Every quarter |
| Annual Work Experience each adult | Every quarter AIS* |
| **JOB SATISFACTION (head of household)** | |
| Attitudes toward work | Pre<br>4th<br>8th<br>12th |
| Job expectations | Pre<br>4th<br>8th<br>12th |
| Job satisfaction and job satisfaction scale | Pre<br>4th<br>8th<br>12th |
| Job alienation | 12th |
| **JOB TRAINING PROGRAMS** | |
| Head | 2nd<br>4th<br>8th<br>12th |
| Wife of Head | 2nd |
| **LEISURE, HOBBIES, VACATIONS** | |
| Family leisure activity | (see FAMILY) |
| Leisure time spent with children | (see FAMILY) |
| Leisure activity - wife | 3rd<br>7th<br>11th |

*Annual Income Supplement.

| | Specific Quarter Asked |
|---|---|
| - husband | 3rd<br>7th<br>11th |
| Sports and hobbies - head | 7th<br>11th |
| Vacations - head | 7th<br>11th |
| MEDIA | |
| TV, Radio, Newspapers, Magazines - general | 3rd |
| Newspapers, Magazines, Books - head | 7th<br>11th |
| TV, Radio, Newspapers - Spouse | 7th<br>11th |
| MEDICAL AND DENTAL | |
| Attitudes toward - head | 2nd<br>6th |
| Health practices - general | 2nd<br>6th<br>10th |
| Medical care adults (excluding pregnancy) | 2nd<br>6th<br>10th |
| Medical care (pregnancy, wife only) | 2nd<br>6th<br>10th |
| Medical care (children) | 2nd<br>8th<br>12th |
| Long-term illnesses or disabilities (adults) | 2nd<br>6th<br>10th |
| Long-term illnesses or disabilities (children) | 2nd<br>8th<br>12th |
| Hospitalization (adults) | 2nd<br>6th<br>10th |

| | Specific Quarter Asked |
|---|---|
| Hospitalization (children) | 2nd<br>8th<br>12th |
| Psychosomatic | 7th<br>10th |
| Dental care (adults) | 2nd<br>6th<br>10th |
| Dental care (children) | 2nd<br>8th<br>12th |
| ORGANIZATIONAL INVOLVEMENT | |
| General - family | 3rd |
| Memberships - Head | Pre<br>3rd<br>5th<br>9th |
| - spouse | 3rd<br>5th<br>9th |
| - children | 3rd<br>5th<br>9th |
| PUBLIC ASSISTANCE | (see WELFARE) |
| RESIDENTIAL MOBILITY | 4th<br>8th<br>10th |
| Marriage termination if related to mobility | 10th |
| Mobility follow-up | special |
| WELFARE, PUBLIC ASSISTANCE | -- |
| - amount | Every quarter<br>AIS |
| - detail | Pre<br>4th<br>11th<br>12th |

| | Specific Quarter Asked |
|---|---|
| Welfare status of head's family at age 16 | 11th<br>12th |
| Welfare status of spouse's family at age 16 | 11th<br>12th |
| Family welfare status during experiment (applicants) | 11th<br>12th |
| Family welfare status during experiment (non-applicants) | 11th<br>12th |
| Family welfare status prior to experiment | 11th<br>12th |

4. FOLLOW-UP INTERVIEW

Respondents Understanding of Experiment

Budgeting of Faming Income

- Family Budgeting - general
- Family budgeting - during experiment
- Effect of interviews on budgeting
- Effect of payments on budgeting

Participation in Quarterly Interviews versus Receiving Payments

- Effect of payments on personal and public relationships*
- Effect of interviews on personal and public relationships
- Perception of amount, accuracy, timing of payments*
- Effect of payments on helping others financially*
- Cheating*
- Payments received*
- Perception of accuracy of reported income
- Perception of present and future financial status
- Effect of end of payments on welfare status*
- Preparation for interview questions
- Expected responses to interview questions
- Perception of rationale behind certain questions
- Estimate of influence of interview questions
- Reasons for refusal to answer certain questions
- Perception of clarity of questions
- Extent of discussion of questions outside household
- Knowledge and perception of relationship between payments and interviews*
- Satisfaction with help received from the payments*
- Satisfaction with the interviews and explanations given for them*

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Asked of experimentals only.

5. SPECIAL DATA FILES

Annual Income Supplement

Special Interview for Attrited Units

Screening Interview

Payments File